ESCAP MEETING NO. 1 - 12/08/99

AGENDA

Kathleen P Zveare
12/02/1999 10:00 AM

To: Kenneth Prewitt, William G Barron Jr, Nancy A Potok, Paula Jane Schneider, Cynthia Z F Clark, Nancy M Gordon, John H Thompson, Preston J Waite, Robert E Fay III, Howard R Hogan, Ruth Ann Killion, John F Long, Susan Miskura
cc: Maria E Urrutia, Fay F Nash, Phyllis A Bonnette, Patricia E Curran, Ellen Lee, Betty Ann Saucier, Jeannette D Greene, Margaret A Applekamp, Jane F Green, Sue A Kent, Mary A Cochran, Linda A Hiner, Carnelle E Sligh, Lois M Kline, Angela Frazier, Linda K Bonney
Subject: Meetings for ESCAP

You should have received a memo (attached) letting you know that we would be contacting you about the Executive Steering Committee for A.C.E. Policy meetings.

The meetings will take place the 2nd and 4th Wednesdays starting December 8 from 10-11:30 in Rm. 2412/3.

Attendees:

K. Prewitt
B. Barron
N. Potok
P. Schneider
C. Clark
N. Gordon
J. Thompson
J. Waite
B. Fay
C. Bush
H. Hogan
R. Killion
J. Long
S. Miskura

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ESCAP MEETING NO. 1 - 12/08/99

HANDOUTS

Charter for the Executive Steering Committee for A.C.E. Policy
November 26, 1999

The Executive Steering Committee for A.C.E. Policy (ESCAP) is established to advise the Director in determining policy for the Accuracy and Coverage Evaluation (A.C.E.) and the integration of A.C.E. results into the census for all purposes except Congressional reapportionment.

The ESCAP will (1) address policy issues that may arise based on internal or external concerns; and (2) review decisions referred to it by technical staff. After discussion, the ESCAP will provide either a consensus recommendation or a set of options to the Director, who makes the final decision on all such issues for the Census Bureau.

In order to operate effectively, the ESCAP will:

1. Assure that operational and technical decisions with policy implications are referred for examination by the Committee. In order to do this, the Committee will become familiar with the field, processing, and estimation operations for A.C.E. and for the use of A.C.E. data in correcting the census counts.

2. Review documentation regarding the feasibility of correcting census counts.

3. Assure consideration of issues that may affect future census-taking.

The ESCAP will be chaired by John Thompson. The full membership of the Committee is as follows:

Kenneth Prewitt, Director, US Bureau of the Census (ex-officio)
Bill Barron
Nancy Potok
Paula Schneider
Cynthia Clark
Nancy Gordon
John Thompson, Chair
Jay Waite
Bob Fay
Howard Hogan
Ruth Ann Killion
John Long
Susan Miskura

The 1990 Post-Enumeration Survey: An Overview

HOWARD HOGAN*

The question of Census undercount has recently gained considerable attention. The 1990 Post-Enumeration Survey (PES) constituted the major vehicle for measuring coverage by area of the 1990 Decennial Census. It was designed to be used to adjust the Census enumeration. This article discusses the background of the survey, the sampling plan, the methods used to measure Census omissions and Census erroneous enumerations, the treatment of nonresponse, the use of dual-system estimation to estimate the total population by post-strata, and the use of these estimates to calculate adjusted Census data.

KEY WORDS: Census; Dual-System Estimation; Matching; Undercount.

1. INTRODUCTION

Every census of population misses some people. Starting in the 1950s, comparisons of the U.S. Census counts to vital records and other aggregate data have shown an undercount by age, sex, and race (Coale 1955; Siegel 1974; U.S. Bureau of the Census 1988). These ›graphic analysis studies have been useful at the .onal level.

Another approach is to take a sample of areas and devote enough resources to produce a very accurate count. Doing this would give an estimate of population at an aggregate level to compare with the Census count. This method can be used to make estimates for states and local areas. However, a very large sample would be required so that the variance would be small compared with the Census undercount. Matching between this second enumeration and the Census allows one to control the variance further, since one needs to only weight-up the omissions and the erroneous inclusions. However, even a very careful enumeration will miss some people. When this method was tried in 1950, it measured an undercount well below that produced by demographic analysis (U.S. Bureau of the Census, 1960). From this experience grew the methods underlying the current Post-Enumeration Survey (PES), based on the ideas of dual-system estimation.

*Howard Hogan is Chief, Undercount Research Staff, Statistical Research Division, Bureau of the Census, Washington, DC 20233. This article reports the general results of research undertaken by the Census Bureau staff. Many statisticians have contributed to the design of the 1990 PES. Special mention must be made of Nicholas Alberti, ...hilders, Gregg Diffendal, Cary Isaki, John H. Thompson, and ...olter. The author greatly benefited from the comments of ...n R. Bell and the referees in making the article more readable. The views expressed are attributed to the author and do not necessarily reflect those of the Census Bureau.

The dual-system model used to estimate the true population conceptualizes each person as either in or not in the Census enumeration, as well as either in or not in the PES.

PES	*Census Enumeration* Total	In	Out
Total	N_{++}	N_{+1}	N_{+2}
In	N_{1+}	N_{11}	N_{12}
Out	N_{2+}	N_{21}	N_{22}

All cells are conceptually observable except for N_{22}, and, of course, any of the marginal totals that include N_{22}. The model assumes independence between the Census and the PES. This means that the probability of being in the ijth cell, p_{ij}, is the product of the marginal probabilities, $p_{i+}p_{+j}$. With this assumption, one can estimate the total population, N_{++}, as

$$N_{++} = (N_{+1})(N_{1+})/N_{11}.$$

The independence assumption can fail either due to causal dependence between the two systems or due to heterogeneity in the population. Causal dependence occurs when the event of an individual's inclusion or exclusion from one system affects the probability of their inclusion in the other system. For example, some people who did answer the Census may not cooperate with the PES, thinking that they had helped enough. However, even if $p_{ij} = p_{i+}p_{+j}$ is true for all individuals (causal independence), the independence assumption can be violated by heterogeneity. The probabilities p_{i+} and p_{+j} must be the same for all individuals, although not necessarily equal to each other. For example, the independence assumption is violated if some people avoid the Census and also tend to avoid the PES. Failure of the independence assumption for either reason results in correlation bias [see Hogan and Wolter (1988) and Wolter (1986)].

Even if independence holds, the model will fail if the people are placed in wrong cells, that is, when there are errors in matching the two systems. Clerks may make errors. Some people in the PES may mis-report their residence on Census Day and so be left unmatched and thus misclassified as not in the Census.

Both the demographic analysis estimates and the PES were developed to provide information to the data users of the probable magnitude of the coverage error so that the statistics could be used properly, as well as to Census planners so that steps could be taken to reduce these errors in future censuses. Soon, statisticians and politicians raised the issue of whether it was possible to correct the errors by statistical adjustment [see Keyfitz (1979)]

In 1980, a number of lawsuits challenged the Census Bureau to correct the original 1980 Census enumeration for undercount. The Census Bureau answered that the available methods were not accurate enough to improve the original enumeration. In one suit that was settled on its merits, the court decided that the Bureau had a reasonable basis for deciding that an accurate adjustment was not possible for the 1980 Census. [See Ericksen and Kadane (1985) and Freedman and Navidi (1986) with their respective discussions for a summary of the controversy surrounding the 1980 Census.]

During the 1980s, the Census Bureau embarked on research to improve the methods to measure and possibly adjust for the undercount in the 1990 Census. In 1987, the Department of Commerce, of which the Census Bureau is part, announced a decision not to correct the 1990 enumeration for coverage error. The Department argued that adjustment would introduce more error than it solved and might divert resources needed for the basic Census enumeration. The Department was sued by a coalition of states, cities, and organizations led by New York City. In July 1989, on the eve of the trial, the Department and the plaintiffs reached an agreement. According to the agreement, the Bureau of the Census would conduct a post-enumeration survey and prepare for an adjustment. The Department of Commerce would publish guidelines for making a decision on whether to adjust and would name a panel of eight experts to advise the Secretary of Commerce on the decision. The agreement set July 15, 1991 as the deadline for the adjustment process and the announcement of the decision. Later that year, the Secretary of Commerce appointed John Tukey, Eugene Ericksen, William Kruskal, Kirk Wolter, Kenneth Wachter, V. Lance Tarrance, Michael McGehee, and Leo Estrada to the panel. The guidelines for making the decision were published in the *Federal Register* in March 1990 (U.S. Department of Commerce 1990). Among the guidelines was a requirement that the PES procedures be prespecified, that is, established and made public before the PES was conducted and the resulting data were analyzed.

The 1990 PES consisted of two parts. The first was a sample of the population, known as the P sample. The proportion of the P sample that was included in the Census estimated the proportion of the total population that was included in the Census. This is a measure of gross undercoverage. The proportion of the sample that is included in the Census is determined by matching the people in this sample to the Census records. If the Census contains out-of-scope records (erroneous enumerations), one needs a sample of the Census enumerations to estimate the number of such records. This sample is known as the E sample. The records are checked against the Census itself to determine the extent of duplication. They are also reinterviewed to determine, for example, the extent of fictitious enumerations, or inclusions by the Census of people born after the Census reference day. The principal steps in PES processing are an initial interview, an initial match to the Census, a follow-up interview of problem cases, and a final match. The estimation steps include missing data adjustment, direct estimation of the population by poststrata, and a regression smoothing to reduce variance. The results of this process are applied to the Census figures to form a synthetic estimate at the lowest level of Census geography, the block.

The PES and adjustment process were completed by July 15. The results were evaluated by the U.S. Bureau of the Census, by the panel of eight, and by the Secretary of Commerce and his staff. The Director of the Census recommended that the Census be adjusted. The panel of eight split, with Tukey, Ericksen, Wolter, and Estrada favoring adjustment and Kruskal, Wachter, Tarrance, and McGehee recommending against adjustment. On July 15, the Secretary of Commerce announced his decision, which was not to adjust the Census. His reasoning is set forth in the *Federal Register* (U.S. Department of Commerce 1991). The plaintiffs then returned to court, where, as of this writing, the matter still stands.

The goal of this article, however, is not to debate the issue of adjustment. Rather, it is to describe the design and principal operations of the PES. In addition to its importance for public policy, the PES presented a number of unusual statistical challenges. It attempted to measure something that is small, the net undercount for a group or area, by carefully balancing larger gross components: gross omissions and gross erroneous inclusions. It was designed specifically to support a set of synthetic estimates at the local level. It was conducted under a tight schedule and a requirement of prespecification unusual for a survey of this size and complexity.

2. STRATIFICATION AND SAMPLING

The PES was designed to support a set of synthetic estimates for local areas. The population was divided into poststrata. The PES estimates the true population for each of these groups. Thus one can calculate the ratio of the PES estimate of the true population to the Census count. This ratio is called the adjustment factor and is the basis for the synthetic estimator.

Poststrata were formed to help the PES meet the independence assumptions required for unbiased estimation using the dual-system estimator. This assumption requires that all people have the same probability of being counted in the Census. We know that, for the total population, this is not true. The demographic analysis estimates have shown that the Census has a persistent pattern of undercount by age, race, and sex. The difficulty in taking a Census, and the kinds of errors, differ for central cities, suburban areas, small towns, and rural areas. Previous research has shown that minority renters are especially difficult to count (Isaki, Schultz, Smith, and Diffendal 1987). One might also suspect that people of different areas of the country might have different inclusion probabilities not easily reflected in other variables. For example, a rural area in the South is quite different from a rural area in the Midwest. Poststrata were formed to classify persons into groups that were as much alike as possible with respect to their Census inclusion probability.

Table 1 gives the variables used to form the poststrata. After combining cells to avoid poststrata representing very small populations, we retained 115 poststrata groups each with 12 age/sex categories. An additional poststrata group was created for American Indians living on reservations. In all, there were 1,392 poststrata. The dual-system estimator was used to estimate the total number of persons in each group separately, thereby preserving as nearly as possible the assumption of equal inclusion probability.

The sample was first allocated to the 54 geographic areas created by the cross-classification of the division and the place/size categories. It was then allocated to achieve a minimum constant coefficient of variation for the estimate of population of each area. This step used estimates derived from the 1980 PES about the level and pattern of undercount. Census data from 1980 were then used to classify Census tracts within these 54 areas according to whether they were primarily (more than 40%) Black, primarily Hispanic, or not primarily made up of either group. This was done only for the purpose of sampling. The estimation poststrata, on the other hand, used each individual's (1990) reported race. In urban areas separate sampling strata were created for renters and owners. After combining cells with small expected populations, 100 sampling strata were defined. Additional sampling strata were formed for the American Indian reservations.

The primary sampling unit for the 1990 PES was the block cluster composed of either a block or a collection of blocks. A sample of 5,300 block clusters was chosen. The same blocks were included in the P (population) sample and the E (enumeration) sample. The E sample consisted of all Census enumerations, correct or incorrect, in the sample blocks. The P sample consisted of all people living in housing units and noninstitutional group quarters in the sample blocks at the time of the PES interview, about 172,000 units. A few groups were excluded from the PES sampling frame: people living in institutions, military personnel living in barracks, and people living in remote rural Alaska. Also, the population defined by the Census Street/Shelter operation, "S-night," was excluded.

Table 1. Variables Used in Poststratification

Race/origin:	Black, non-Black Hispanic, Asian and Pacific Islanders, and all others
Age:	0–9, 10–19, 20–29, 30–44, 45–64, 65+
Sex:	Male, female
Census division:	New England, Middle Atlantic, South Atlantic, East South Central, West South Central, East North Central, West North Central, Mountain, Pacific
Place/size:	Central City of Major Primary Metropolitan Statistical Areas (PMSA's)
	Central City of Large Metropolitan Statistical Areas (MSA's) (with at least one city with population of 250,000 or more)
	Central City of Small MSA
	PMSA or large MSA: Not Central City
	Small MSA: Not Central City
	Non-MSA incorporated places with population of 10,000 or more
	All others
Tenure:	Owner, renter

Many of the blocks defined by Census geography contain no or very few housing units. These include many rural blocks, blocks in industrial and business districts, as well as "blocks" defined by highway cloverleafs and median strips. In order to control the sample size, it was necessary for the sample to limit such "small blocks," which were defined as blocks with fewer than three housing units in the pre- Census counts. A much lower sampling rate was used for these blocks, so that people found in these blocks received a high sampling weight. [See Alberti et al. (1988) and Woltman, Alberti, and Moriarity (1988) for a complete description of the sampling.]

3. LISTING AND INTERVIEWING

PES field work began in February when permanent Census Bureau interviewers visited each sample block to list all housing units. PES interviewing started in late June 1990, after the Census had completed enumerating most housing units. Interviewing was largely conducted by former Census enumerators. However, to help ensure operational independence from the Census, the PES was managed out of 12 regional centers rather than out of the local District Offices that conducted the Census enumeration.

The people living in the sample blocks at the time of the PES constituted the P sample. Therefore, the interviewers asked about those living there at the time of the PES interview, rather than trying to reconstruct the household as of April 1, Census Day. It is difficult enough to include the "hard-to-count" population where they are living at the time of the interview. It is unreasonable to expect to identify them months after they have left. Questions were asked about persons who lived at the sample address on April 1, but who were not still living there. However, this information was used only to resolve cases from the E sample.

The PES interview asked for the same basic demographic information as the Census: name, relationship, sex, marital status, race, date of birth, and Hispanic origin. In addition, it asked a battery of questions to help with the matching. These include questions about other names that the person might use. Since the Census questionnaires are indexed only by address, the PES asks several questions about exactly where the person lived on April 1. Table 2 lists some of the questions from the PES questionnaire. The names of the neighbors were used together with cross streets and landmarks to verify that the clerks were searching the Census records of the correct block.

4. MATCHING

To determine whether a person in the P sample was enumerated, one needs to match the P sample records to the Census records, which are only indexed by geographic location. The first stage in matching was done

Table 2. Selected Items from PES Questionnaire

1. What is the full name of each person now living or staying at this address, starting with the name of the household member in whose name the house is owned or rented? (Exclude anyone who has a regular fixed address somewhere else.)
2. I have listed . . . persons. Are there any persons that you have not mentioned for any reason, such as those traveling or temporarily hospitalized?
9. What is . . .'s maiden name?
10. Does . . . ever use another name, such as a nickname (or a name from a previous marriage)?
11. How long has . . . lived or stayed here?
12. What date did . . . move to this address?
13. What was . . .'s address on April 1, 1990?
14. What are *names of the cross streets, roads, highways, or other landmarks* closest to that address?
15. What are the names of two neighbors living near that address?
17. People sometimes have more than one place where they stay. This can cause us to count them more than once. Did (you/any of the people now living here) stay any part of March or April of this year at a college or university, with another relative, at a second home, on a military base or ship, or somewhere else for any reason?
18. What is the exact mailing address here?
20. What is the telephone number here?
21. Are there any persons who lived here on April 1, 1990, who are not living here now?

by using a computer matching system that the Census Bureau has developed over the decade (see Jaro 1989). In addition to the individual characteristics and address information that the Census routinely computerizes, the names of the people enumerated in the sample blocks and surrounding blocks were keyed to assist computer matching.

Clerks reviewed all matched and unmatched cases and corrected errors made by the computer matching. Clerks could take account of relationships, review notes, and decipher handwriting. To aid the clerks, the computer matching system printed out its results by household. The clerks checked the sample block for matches. They also searched each of the surrounding blocks. Alphabetized lists of all people enumerated in surrounding blocks were provided, together with the actual Census questionnaires.

For the PES, a person was considered enumerated by the Census if his or her name was listed on an individual Census record that was included as part of the count of the population. A person was considered omitted from the Census if he or she should have been part of that count but was not.

The concept of "Correct Address Matching" or "Unique Address Matching" was used. The matching classified persons as enumerated only if they were counted at the location where they should have been counted, according to the information they provide. For example, Census rules required that college students away at school be enumerated at the university, not at their parents' home. The PES classified students as "enumerated" only if they were counted at the university. Otherwise, the students were classified as "omitted," even if they were counted at home. To measure net undercount, the estimator must classify the enumeration at home as erroneous and subtract it from the Census. In this example, there would be one omission (at the university) and one erroneous enumeration (at home). The two net out if the two locations (home and university) are part of the same poststratum.

If a person reported that he or she lived at a given address, then the matching classified him or her as correctly enumerated if he or she was counted anywhere in the block that included that address. It also classified him or her as correctly enumerated if he or she was counted in a group of surrounding blocks. The group of blocks whose enumerations were searched is known as the search area. However, there is a limit as to how far the matching process can search. If a Census operation coded the address across town, the matching counted the person as missed. Census enumerations that were outside the search area of the true location were classified as erroneous, so that the estimate of net error would not be inflated.

Another concept used in matching was that of "Sufficient Information for Matching." When a match was found, it was easy to say that the person was enumerated, although not necessarily *correctly* enumerated. Finding no match did not prove that the person was not enumerated. For example, the clerk may not have looked in the correct place. The cases where it was determined that there was insufficient information to classify whether the person was enumerated were called "Unresolved" and were treated as missing data (see Section 6). Given discretion, there is a tendency for clerks to classify cases that match as "Resolved: Enumerated" and cases that do not match as "Unresolved." This can create a strong bias. Therefore, the rules that classified cases as "Sufficient Information for Matching" were in general applied before the matching began. If the clerks found a match, they classified the case as correctly enumerated. Equally important, if they did not find a match, the case was classified as omitted. Strict applications of these rules lead to some cases being considered "Unresolved" even though a search of the Census records might locate a match.

There was an elaborate classification system. For example, the "Omitted" cases are classified into:

- Within household nonmatches
- Housing unit included but whole household nonmatches
- Structure included but household and housing unit nonmatches
- Whole structure nonmatches
- Census processing error (i.e., questionnaire returned but not counted in the Census)

An initial match code was assigned before follow-up and a final match code after follow-up. The before-follow-up match codes were used to predict enumeration status for cases that could not be interviewed during follow-up. The final match codes provide important information to study the nature of Census errors beyond the question of net undercount.

The computer matching only worked for people who were living in the sample or surrounding blocks on Census Day. Outside these areas, the names were not keyed. Clerks matched those who moved since April 1. Clerks assigned the reported Census Day address to a Census block. Microfilm copies of the Census questionnaires, which show names, were printed and searched by the clerks. Clerks were required to confirm that they were searching in the correct area before the matching process classified a person as not-enumerated. There can be several problems. For example, the respondent may report correctly that he was living at "1102 Elm," but the interviewer may record, "1012 Elm." Even if the interviewer records the response correctly, there may be an Elm Street, an Elm Avenue, an Elm Court, and an Elm Terrace. If the clerks found the cross streets, located the neighbors, or found any of the other household members, the case could be coded not-enumerated with confidence. If they did not find any of this confirming information, then they tried to recode the address and, if still unsuccessful, sent the case to follow-up to get more information on the address. If after follow-up the address still cannot be confirmed, the case is classified "unresolved."

Most P-sample cases that were not matched were sent to the field for follow-up. These include the following cases:

Whole household nonmatches with conflicting information: cases where the Census reports one family (the "Emersons") as having lived at the address on April 1, but the PES interview reported another family (the "Petersons") as having lived there on Census Day

Whole household nonmatches without conflicting information: cases where the PES interview has indicated that the family lived at the address, but the Census was imputed, did not get names, or listed the house as "Vacant"

Whole housing unit nonmatches: cases where the PES interview indicated that the family lived at the address, but the Census enumeration omitted the unit or coded it outside the search area

Nonmatched proxy interviews: cases where the initial PES interview was with a neighbor or other nonhousehold member

Nonmatched movers

Pairs of possible matches: P-sample cases and Census enumerations that might refer to the same individual, but more information is needed

Follow-up served several purposes. Nonmatched movers were sent to follow-up to get better geographic information. Follow-up helped detect cases where the initial PES interview was fabricated. Experience from the 1986 PES test has shown that sending the whole household nonmatched P-sample cases out for reinterview together with the nonmatched E-sample cases from the same housing unit leads to more accurate reporting of both (see Hogan and Wolter 1988). Nonmatched cases where other members of the household matched were not sent to follow-up provided the information was reported by a household member. Not following up these nonmatches reduced the follow-up workload and allowed the limited pool of better trained interviewers to concentrate on the other cases. Further, any changes in April 1 addresses reported in follow-up had to follow the concept of "Unique Address Matching." Some of the nonmatched people would give different but not necessarily more accurate information, as would some of the matched people if they were asked. After follow-up, clerks assigned a final match code.

5. MEASURING ERRONEOUS ENUMERATIONS

The process described so far measured only part of coverage errors: the proportion of people missed by the Census. The E sample measured the proportion of erroneous enumerations. Erroneous enumerations include Census duplicates, Census fictitious enumerations, people who were born after Census Day or who died before Census Day, and people who were counted in the wrong place. The E sample consisted of all Census enumerations coded, correctly or incorrectly, to the blocks sampled for the P sample. For purposes of *sampling*, it does not matter where the person, housing unit, or address actually was, only where the Census coded it.

The design treated an enumeration as correct if it is not a duplicate and if, according to the information provided, the person should have been counted either in the sample block or in one of the surrounding blocks that make up the search area. For every enumeration in the E sample that was linked to a P-sample person, interviewers had already asked the questions about where that person lived on Census Day. For every address in the E sample that was also in the P sample, interviewers had already determined its physical location. If the people have reported that they lived there on Census Day, the enumeration is considered correct. But, a person may have already reported that he moved in after Census Day or was away at college. These cases would be coded as erroneous enumerations. Also, during the June PES interview, interviewers had already asked whether there was anyone who lived at the address on Census Day who has left (Question 21). These persons are not in the P sample. However, if they were enumerated in the Census, then they were classified as correctly enumerated, unless a duplicate is found in the search area.

Some people enumerated in the Census were, of course, missed by the PES. Their houses may have been missed, they may simply have been omitted from the PES roster, or they may have left and not been reported by the current resident. Such cases were sent for follow-up interviews. Follow-up interviewers asked the same questions that were asked in the P sample: Where were they living on April 1, 1990? Were they away at college, at a second home? Clerks then applied the same rules that were applied to the P-sample persons to determine the "correct" April 1 usual place of residence. If the PES did not list the building where they were living, the follow-up interviewer marked the location on a map.

Clerks coded the enumeration as erroneous if the building was outside the search area.

The E sample also measured the number of fictitious Census enumerations, "curbstones" in the jargon of the Census Bureau. Proving that someone does not exist is difficult. When the interviewer asked, "*Do you know . . . ?*" as answer of "No" may indicate no more than that the respondent did not know the person, not that the person did not exist. The rules required the interviewer to find several knowledgeable respondents in an effort to determine whether an enumeration was fictitious.

Finally, the Census included enumerations with such sparse data that they did not identify a unique individual. Usually, these were enumerations without names. It would be impossible to match the P sample accurately to these enumerations or to conduct a follow-up interview. These cases were counted as erroneous enumerations. The Census count also includes whole-person imputations, that is cases where the data about an individual were so sparse that another record was substituted. All these cases were classified for PES estimation as not in the Census. Of course, these cases were included in the Census counts when computing net coverage error or applying the adjustment factors.

A special operation processed Census enumerations that occurred after computer matching. This operation presented special challenges in merging the data with the results of the earlier operation and completing the processing in time for follow-up. However, it presented no new conceptual problems. A small number of Census cases were added to the Census files too late to include in the PES processing, but are included in the Census counts. These cases introduce an upward bias to the dual-system estimate if they either should have matched or should have been classified as erroneous enumerations.

6. ESTIMATION

In order to calculate the dual-system estimate for each poststrata, one must know how many people were counted in the PES only, the Census only, and in both. Missing data make this impossible to determine exactly, either because a person is not assigned to a unique poststrata, or because the person is not assigned to a specific dual-system estimation cell.

P-sample cases with missing data occurred because of initial noninterviews or partial interviews and from failed or incomplete follow-up interviews. For some cases no information was gathered because of initial whole household refusals or temporary absences. For other cases, the interview is complete but the reported April 1 address could not be assigned to a Census block. In the E sample, noninterviews arose only from the follow-up, since, as noted above, "nonresponse" Census enumerations are treated as erroneously enumerated. Still, the categories of noninterviews are complex. For example, the interviewer may have determined that the enumeration referred to a real person, but learned nothing else. In an otherwise complete interview, the interviewer may have failed to draw a map showing whether the housing unit was within the search area. The nonresponse adjustment mechanism reflects this complexity.

The missing data adjustment began by reweighting response cases for the whole-household noninterviews. Reweighting was done within block where possible. Next, the process imputed for missing demographic characteristics so that each case could be assigned to a poststratum. For example, if race was missing, it was imputed based on the race of other members of the household or that of neighbors. If age was missing, it was imputed based on the distribution of the response cases with similar other characteristics.

To account for unresolved enumeration status, a large logistic regression model was fit to P-sample data for which enumeration status was observed. This model was used to predict the probability of correctly enumerated versus that of omitted from the Census for unresolved P-sample cases. A separate logistic regression model was fit to resolved E-sample individuals to predict probability of correctly enumerated versus erroneously enumerated for unresolved E-sample cases. The model allowed the probability of having been enumerated to depend on the before-follow-up match code assigned by clerks and on other covariates which included demographic and geographic characteristics. Some effects were assumed common to all individuals, and some effects were allowed to vary across groups having common before-follow-up match status. Parameters for individual match-code groups were estimated using a hierarchical model that "borrows strength" across groups. The model adapted linear-model techniques described in Braun, Jones, Rubin, and Thayer (1983) and Dempster, Rubin, and Tsutakawa (1981) to the logistic-regression setting. For a more complete account, see Diffendal and Belin (1991).

The factors controlled in both models included the individual's age group, sex, race, and origin, whether the household owned or rented, and whether the housing unit was in a single-unit or multiple-unit building. The models also took into account whether the case went to follow-up, the match status before follow-up, and whether item characteristics had been imputed. Geographic effects were reflected by including the sampling stratum indicators.

The model for P-sample cases additionally controlled for mover/nonmover status and whether the PES interview was conducted with a proxy respondent. The model for E-sample cases additionally controlled for the source of the unit in Census lists, type of mailing address, and whether the Census form was returned by mail. Generating predicted probabilities for unresolved cases conditioned on these factors mitigates biases in undercount estimates that would arise if unresolved cases were ignored or were treated like resolved cases.

Dual-system estimates were made for each of the 1,392 poststrata. Note that in the dual-system model, the marginal total, N_{+1}, is the number of distinct and identifiable people in the Census. This differs from the official Census count which includes duplicates, fictitious cases, and other erroneous inclusions. These are measured by the E sample and subtracted before forming the estimates. Specifically, the estimator takes the following form within poststrata:

$$\hat{N}_{++} = (\hat{N}_p/\hat{M})(N_c - \mathrm{II})(1 - \widehat{EE}/\hat{N}_e),$$

where $\hat{N}_{++}$ = dual-system estimate of the population, $\hat{N}_p$ = weighted P-sample total, N_c = census count, II = number of whole-person Census imputations, $\widehat{EE}$ = weighted estimate of E-sample erroneous enumerations, $\hat{N}_e$ = weighted E-sample total, and, $\hat{M}$ = weighted estimate of P-sample matches.

The difference between the estimated true population N_{++} and the Census count N_c (without removing erroneous enumerations) estimates the net Census undercount. The ratio of the estimated true population $\hat{N}_{++}$ to the Census count N_c is the adjustment factor.

7. SMOOTHING AND CARRYING DOWN

It was anticipated that many of the 1,392 poststrata adjustment factors would have coefficients of variation too high to be useful for adjustment. One way to reduce the variance would be to form fewer poststrata, that is, to assume homogeneity across broader categories. Instead, a regression approach was adopted. A regression was used to predict the adjustment factor N_{++}/N_c for each poststratum. The regression-predicted factor was then combined with the observed factor to form the smoothed factor.

The model for the adjustment factor is

$$\mathbf{Y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{w} + \mathbf{e},$$

where $\mathbf{Y}$ = vector of observed adjustment factor by poststratum, $\mathbf{X}$ = matrix of regressed variables, $\boldsymbol{\beta}$ = vector of regression parameters, $\mathbf{w}$ = model error, assumed $N(0, \sigma^2\mathbf{I})$, and $\mathbf{e}$ = sampling error, assumed $N(0, \mathbf{V})$, where $\mathbf{V}$ is the sampling error covariance matrix.

The error terms, $\mathbf{w}$ and $\mathbf{e}$, are assumed to be independent. The observed adjustment factors for the 1,392 poststrata were divided into four Census regions and the American Indian Reservation strata. Separate regression fits were made for the five groups.

The regression used indicators for race (Black, Asian), Hispanic origin, age category, tenure, Census division, and place/size category. Interactions were allowed between race and place/size, between age/sex/race, and between age/sex/tenure. Several other regression variables were formed as measures of degrees of census-taking difficulty. The proportion of people in the poststratum enumerated on questionnaires returned by mail measured public cooperation with the Census. The proportion of Census whole-person-substitution measured the extent to which the Census relied on imputation. Another variable indicated the proportion of enumeration conducted using traditional door-to-door enumeration, a method used primarily in remote rural areas.

Indicators for race, age, and tenure were forced to enter the model, with the other variables selected for the model based on their predictive power. The other regression variables were selected using a best-subsets regression (Furnival and Wilson 1974). This approach was chosen over more subjective methods of variable selection to meet the requirement of prespecification.

For any subset of regression variables $\mathbf{X}$, an iterative procedure was used to estimate $\boldsymbol{\beta}$ and σ^2. Given an estimate of σ^2 and an estimate of $\mathbf{V}$, we can compute $\hat{\boldsymbol{\Sigma}} = (\hat{\mathbf{V}} + \hat{\sigma}^2\mathbf{I})$ and the generalized least squares estimate

$$\hat{\boldsymbol{\beta}} = (\mathbf{X}'\hat{\boldsymbol{\Sigma}}^{-1}\mathbf{X})^{-1}(\mathbf{X}'\hat{\boldsymbol{\Sigma}}^{-1}\mathbf{Y}).$$

The maximum-likelihood estimator is the $\hat{\sigma}^2$ and $\hat{\boldsymbol{\beta}}$ that maximize the likelihood of the model. Although $\hat{\mathbf{V}}$ is an estimate, we assumed that it was known.

Experience from earlier tests and theoretical considerations suggested that the sampling variances would be high for adjustment factors either much above one (large estimated undercounts) or much below one (large estimated overcounts). If the sample estimated variances were related only to the true adjustment factors, this would have been appropriately accounted for in the generalized least squares fitting and smoothing. However, it was likely that the sampling errors of the estimated variances would be related to the sampling errors in the estimated adjustment factors. This could have resulted in under- or overweighting of certain factors. For this reason, the variances were presmoothed. The following model was postulated for the variances:

$$n_i v_i/(1 + CV_i^2) = b_0 + b_1 W_i + b_2 AI_{1i} + b_3 AI_{2i} + b_4 \mathrm{Min}_i,$$

where v_i = true variance of the raw adjustment factor, n_i = P-sample number of people in the ith poststratum, CV_i = coefficient of variation of the P-sample person weights, W_i = an initial regression approximation to the adjustment factor, constrained to be at least 1.00, AI_{1i} = age indicator for ages 0 to 19, AI_{2i} = age indicator for ages 20 to 44, and Min_i = Variable indicating the proportion of minority in the ith poststratum.

The term W_i is included to account for the correlation between the true variance and the true adjustment factor. It is estimated using the same variables and best-subsets regression program as that used for the final estimates but without iteration and, of course, using the sample estimated variances.

The variance model was fit by region using least squares with weights proportional to the square root of n_i. Coefficients with t-statistics less than two were set to zero and the model was refit. This model seemed to work fine in pulling up the low variances. However, for a few points with high sample variances, the model predicted much lower variances than the initial PES estimates, that is, there were a few very large outliers. To lessen the weight given to such points, any point with a Studentized residual greater than four was omitted from the variance modeling and the sample estimated variance was used. Two iterations were used to identify outliers. The original correlations were used with the presmoothed variances to compute covariances to complete the estimate of V. A further description of the smoothing process is found in Isaki et al. (1987, 1988, 1991).

The final estimates of β and σ^2 were computed using the regression estimates of V. Given estimates of β and σ^2, the smoothed adjustment factors are computed by

$$\hat{y} = X\hat{\beta} + \hat{\sigma}^2 I \hat{\Sigma}^{-1} (Y - X\beta).$$

Were there no covariances, this would be equivalent to adding back to the regression estimate a part of the residual, with the part being proportional to the model variance and inversely proportional to the sampling variance. Since covariances were involved, a linear combination of many residuals is added to the regression estimate. The actual smoothed factor can lie outside the interval between the observed and regression adjustment factor. As a final step, the smoothed factors were ratio-adjusted so that for each Census region, the smoothed undercount equaled the directly-estimated undercount.

Distribution of the estimated undercount geographically below the poststratum level was done by multiplying the poststrata adjustment factors by Census counts for each poststrata in each block in the Census. The block was used to ensure that all subsequent tabulations based on the adjustment are consistent when aggregated. The Census counts for groups excluded from the PES frame, for example, the institutional population, remain unchanged.

This process will generally not produce whole numbers of persons. Neither the Census tabulation and publication system nor the majority of Census users are prepared to deal with noninteger numbers of persons. Fractions were rounded either up or down to a whole person, using a controlled rounding procedure that ensures that the poststrata within a block as well as the total for any block are not rounded up or down by more than one. The totals by poststrata for states were controlled to the level of precision of the computer, roughly 10 people.

The PES poststrata employed broad age categories, such as 0–9, 10–19. Except for American Indians on reservations, it employed only four race/origin categories: Black, non-Black Hispanic, Asian and Pacific Islanders, and all others. In order to reflect the count adjustment in individual records for the Census blocks, imputation was done using a hot-deck procedure similar to that used for other Census missing data. For example, to add a 0–9-year-old Hispanic in a predominantly Mexican-American origin block, the process imputed exact age (say, 5) and, usually, imputed the person to be Mexican-American. If there was no one of a given ethnic origin, age group, or sex, in a given block, the PES could not add anyone of that origin, age, or sex to that block. If a net overcount was measure, a record was added with negative weight. This was done to avoid deleting individual records from within households. After the count adjustment record was imputed, the adjusted files were tabulated.

As mentioned above, the Secretary of Commerce decided not to use the results of the PES to adjust the 1990 Census. However, work on the PES has not stopped. The data continue to be evaluated and analyzed. Information from the PES may be incorporated into the Census Bureau's postcensal estimates of population. The results of the PES can also help in guiding the 2000 Census planning effort, both for what it can say about the difficulty of traditional enumeration approaches and for what it can say about the possibilities of statistical approaches. The PES has provided a data set that analysts and survey statisticians will be studying for many years.

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Draft Summary Report of DSE Technical Analysis Concepts, Assumptions, and Issues

11/29/99--SMO/RF

BACKGROUND

The 1991 Post Enumeration Survey (PES) was a 157,000 housing unit measurement survey that used a statistical technique know as Dual System Estimation (DSE), or "capture/recapture." Capture/recapture is often applied to estimating numbers of fish in ponds, lakes, and oceans. Fish are caught, tagged, thrown back and some are recaught in a second catch. In the analogy, the first catch corresponds to the census, the tagging corresponds to names and demographic identifiers useable for matching, and the second catch corresponds to the PES. The two are compared and net differences between the two counts are computed.

Predetermined groupings (poststrata) of people believed to have similar capture probabilities are identified. The so-called adjustment factors are ratios of the number of people in a poststratum counted in the PES to the number counted in the census. These factors are what is applied to the census count to compensate for overcounts of some groups and undercounts of hard-to-enumerate groups. These adjustment factors are to be applied to census counts all the way down to the block level. The difference between a Dual System Estimate (DSE) and the census count is the estimate of the net undercount, which was about 4 million in 1990. The undercount of 4 million persons was error (i.e., bias) in the census. Since PES undercount estimates (DSEs) were based on a sample survey, they too are subject to error. There is sampling error, which reflects the fact that the information came from some and not all of the population. In addition to sampling error, the PES estimates, like the census, are also subject to biases measured using a total error model.

The bureau's model of total error in the 1991 PES allowed for a decomposition of the components of non-sampling error or biases present in the PES estimates. For example, errors in matching, erroneous responses from respondents or fictitious names can bias the undercount estimates. Another type, correlation bias is present when the measurement tool (i.e., the PES) underestimates the number of people missed by the census because they were also missed by the PES. Total error in the PES ranges from about 22 percent of the estimate if correlation bias is included and about 45 percent if it is not. This outcome occurs because correlation bias is negative, meaning that it is underestimating the undercount. Other biases, such as matching error, overestimate the undercount and therefore offset one another. Consequently, it is no single error component that defines the quality of the DSEs but the total error as compared to the large error component present in the census--undercount.

Consequently, at the core of the decision on whether to use the estimates of 1990 census undercount to adjust the census is the question: Which set of numbers is more accurate--the DSEs or the census? That is, in which set of numbers is there less total error? This same question underlies the controversy surrounding the use of adjusted numbers in redistricting and funds allocation planned in 2000. In addressing this question, it is necessary to highlight a number of technical issues that are interleaved with the concept of "error."

The overall purpose of this document is to provide an integrated view of Dual System Estimation by providing (1) a brief description of important concepts, (2) an overview of underlying assumptions and expectations with regard to performance, and (3) a detailed discussion of issues associated with error and their statuses. The final section discusses our conclusions about projections of error in 2000 and recommendations.

CONCEPTS AND DEFINITIONS

1. Variance Model--Error in the Census Effects on A.C.E.

The quality of the census and the quality of the estimates from the A.C.E. are inextricably coupled. In 1998, we designed an error model that underlay assumptions we were making about the variance levels we were anticipating at various geographic levels. Heuristically, the idea is that the components of error in the 2000 census must be comparable to error levels in 1990 for our assumptions about variance to hold. Consequently, we identified those components and error levels which gives us a tool to use for monitoring performance.

- (A) P+E Net Household undercoverage, including movers--This is a composite component. It includes
  - (1) Missed households at enumerated housing units.
  - (2) Missed households in missed housing units at addresses with other included units.
  - (3) Omission of movers.
  - (4) Definitional errors measured by the E-sample, including fictitious enumerations.
  - (5) Insufficient information for matching.
- (B) Within household omission--This represents persons left off the census roster in households correctly enumerated in the census but who were omitted for reasons ranging from misunderstanding to deliberate concealment.
- (C) Missed housing unit at missed address--This includes all P-sample persons, their housing unit, and the street address missed from the search area.
- (D) Duplicate enumeration--This includes duplicates within blocks as well as between blocks in the same search area.
- (E) Geocoding error--This is when housing units is in the MAF as it should be but is assigned to an incorrect block.
- (F) Miscellaneous P+E sample components--This term collects the remaining components of the P- and E-sample estimates. It includes imputations for nonmovers, miscellaneous categories of nonmatch, and E-sample persons imputed to erroneous enumerations.

2. Total Error--Census versus the A.C.E.

The underlying principle behind adjustment is that the total error in the census far exceeds that in the A.C.E. hence leading to a count closer to "truth." The census contains only nonsampling error or bias known as undercount. The A.C.E has both sampling and nonsampling error. At the national level, the sampling error is negligible but the amount of nonsampling error that is not undercount is a highly controversial topic. To demonstrate relative accuracy in the census versus the PES in 1991 and 1992, the bureau developed a total error model. This model identified the components of error and used evaluation results to build the model. It was used to identify " truth" and then to compare census and adjusted counts to truth to demonstrate relative accuracy. Its components were as follows:

(A) Matching Error--The net error in assigning cases the status of enumerated or not enumerated to P-sample persons which occur during the processing of the data.

(B) P-Sample Collection Error--Errors by respondents and interviewers during P-sample data collection such as misreporting whether a respondent has moved between Census Day and the A.C.E. interview or if they have moved, have they reported their previous address accurately.

(C) P-Sample Fabrication Error--An error resulting from the making up of people in P-sample housing units.

(D) E-Sample Collection Error--Errors that occur because respondents don't remember correctly whether they lived at the address on Census Day or interviewers may make errors in administering the questionnaire or in recording

(F) E-Sample Operation Error--Errors in measuring census error during the office processing of the E-sample. An error in the estimation of the number of erroneous enumerations occurs either when an enumeration in the E-sample is designated as erroneous although it is correct, or when an enumeration is designated as correct although it is really erroneous.

(G) Model Bias--In 1991 and 1992, model bias equaled correlation bias (see below).

(H) Ratio Estimator Bias--The empirical DSE is a ratio and therefore is subject to the bias of a ratio.

(I) Sampling Error--The DSE is subject to sampling error because the error components are estimated from sampling.

(J) Imputation Error--Both the E and P-sample have cases with unresolved enumeration status after the matching operation is complete. For these cases, the probability of being enumerated is imputed statistically to compensate for the inability to resolve the case.

3. Missing Data--Effect on the Quality of the A.C.E.

The lower the missing data rate in both the census and the A.C.E., the more accurate (less error) the results are presumed to be. In 1990, imputation rates for the P and E sample were 1.7% and 2.1%, respectively--on the same order of magnitude as the undercount. If the assumptions underlying the imputation model were incorrect, the variation in the estimates could be well beyond that expected from sampling alone. However, because of the high response rates and correspondingly low imputation rates, bias in the PES in 1990 due to imputation was negligible. If we have excessive missing data in the census, we could end up with high variances for the A.C.E. If we have excessive missing data in the A.C.E. we could end up with high biases, increasing total error.

4. Matching Error--Effect on Quality of the A.C.E.

Highly accurate matching is important because matching errors in even a small percentage of cases can significantly affect undercount estimates. Matching error is also a large source of bias in the total error model. If match rates are too low, our biases could become unacceptable. In 1991, Dr. Ken Wachter stated that our estimate for the matching error was too low, because the rematch study "did not, by its nature, expose certain inevitable kinds of matching errors."

5. Erroneous Enumerations in the Census--Effect on Quality of the A.C.E.

Erroneous enumerations include people who died before or were born after census day, fictitious people and pets listed as members of a household, twice counted people as well as people enumerated outside the PES matching area. In 1990, there were a large number of erroneous enumerations in the census and they were differentially distributed. Just like with geocoding errors, problems arise with erroneous enumerations when they cluster. In clustered blocks when the adjustment factor is applied, error level is exacerbated.

6. Correlation Bias

The Dual System Estimate of total population produced by comparing the A.C.E. and the census is a biased estimate because of the error components, e.g., matching error. The DSE can also be biased by correlation bias which has multiple components. The first is that the DSE assumes that a person's participation in the A.C.E. is not affected by his participation in the census (the causal independence assumption). Failure of this assumption can cause bias but is generally not considered problematic because of the operational controls put into place.

The second component occurs because of the variable capture probabilities within a post-stratum. The DSE assumes that within a post-stratum, everyone in the A.C.E. or census has approximately the same capture probability. Generally, if people within a post-stratum have differing capture probabilities, then the DSE underestimates the total population and in most cases would underestimate the undercount. A third and final component are the impossible to count or people missed in both the census and the A.C.E.

There are no direct estimates of either of the last two components, but an estimate for the total is obtained by comparing the A.C.E. estimates to Demographic Analysis (DA) estimates. After estimating the extent of correlation bias, the estimate is added to the total error model and is used to determine target numbers for loss function analysis.

7. Loss Function Analysis

If truth were known, the census count and the adjusted base count could be compared to truth and an appropriate choice made. This is impossible. To approximate that comparison, the bureau in 1991 and 1992 performed loss function analyses. Loss functions are mathematical methods to examine relative loss. They are sometimes used in the apportionment process.

First, the true population is estimated and is called the target population. It is estimated by taking the A.C.E. estimate of population and modifying that estimate based on the estimates of error in the A.C.E. (the components of bias from the total error model). A modeling system must be used to allocate the bias from the evaluation post-strata to sub-levels of geography. Because there is no consensus on what are the appropriate representations of error and how to allocate bias to the target numbers and whether to include correlation bias, the bureau ran a variety of loss functions. To determine whether the differences were real or only due to random error, the bureau conducted hypothesis testing on the loss function differences.

8. Synthetic Assumption

A synthetic adjustment assumes that the probability of being missed by the census is constant for each person within an age, race, Hispanic origin, sex, and tenure category in a geographic area. The adjusted census estimate for the census block is calculated by adding together the estimated adjustments for each post-strata represented in the block. Because of the problems of correcting a census with a survey, adjusted figures cannot be more accurate than the census counts in each block or at all larger aggregates of them. The bottom line is what is the residual heterogeneity within poststrata down to the block level and what is the effect of that heterogeneity on the adjusted estimates both in levels and shares.

Assumptions

Performance expectations for the Accuracy and Coverage Evaluation (A.C.E.) survey are premised on 2000 error levels in the census and A.C.E. error levels being comparable to 1990. These assumptions include variance and bias error component projections.

In 2000, the census may differ from 1990 and could affect variance and bias levels, for example:

- The MAF may not achieve 1990 completeness levels (99% complete) due to a more dynamic populace;
- Although improvements in the data capture system have been made, new issues could arise under census conditions that could increase the amount of missing data;
- Mail response below projected levels could lead to an increase in A.C.E. variance; and
- An increase in the minority proportion over the decade may also contribute to increased variance.

Although modeled on the 1990 PES, the A.C.E. is different in some ways that could affect variance and bias, for example:

- The PES sample was selected using measures of size before final changes to the MAF, resulting in some sampling inefficiencies and potentially, weight variation;
- The treatment of movers is different. Depending on the completeness of data for movers, PES C for 2000 may have somewhat different variance and bias properties than the PES B in 1990;
- The sample size for the A.C.E. is almost doubled over the 1990 PES; and

- The initial household response to the A.C.E. may be lower than 1990.

Therefore, understanding the components of error and their relative effects on variance and bias provides managers with important indicators for monitoring and/or evaluating performance.

__Variance__

A basic premise of the current Census 2000 design is that the use of sampling can reduce or eliminate systemic bias inherent in the traditional census without introducing an unacceptable level of sampling error. Calculation of the sample size based on the 1990 PES implicitly assumes no change in under coverage of the census in 2000 relative to the 1990 undercount and in other aspects affecting variance, that is, the 2000 census would yield results similar in coverage to the 1990 census, and the 2000 A.C.E. would behave similarly to the 1990 PES. Consequently, the following table lays out the error projections upon which our variance assumptions are based and how they may change in 2000:

Potential Change in 1990 Variance Components in 2000

Component	**1990 % Error Rates**	**Expected Performance in 2000**
P+E Net Household/Movers	0.66	Potential increase due to PES C
Within HH Omission	1.64	
Missed Housing Unit	1.19	
Duplicate	1.62	
Geocoding Error	0.34	Definite increase due to less than full search in A.C.E.
Miscellaneous P+E	0.51	

__Biases__

As with variance projections, 2000 nonsampling error projections are premised on the assumption that the 2000 census would yield results similar in coverage to the 1990 census and that the 2000 A.C. E. would behave similarly to the 1990 PES. The following table lays out 1990 PES error levels and how they may change in 2000:

Potential 1990 PES Total Error Changes in 2000

Component	**1990 % Bias Levels**	**Potential Change in 2000**
Matching Error	0.21	Should decrease due to improvements in matching system
P-sample Collection Error	0.31	
P-Sample Fabrication Error	0.02	
E-Sample Collection Error	-.17	
E-Sample Operations Error	0.25	
Correlation (Model) Bias	-.29	May stay about same because massive Ad campaign could reduce but PES C could increase

Ratio Estimator Bias	0.11	Should remain about the same unless over stratification occurs
Sampling Error	0.00	Should decrease due to doubling of sample size but gains could be offset by TES, etc.
Imputation Error	0.00	Likely to increase due to expected lower response rates

ISSUES AND RESPONSES

I. Sample Size

Issue I.1: *In 1990, the PES sample size was marginal for making the decision to adjust. Does doubling the size of the total sample, but only making a minor upwards adjustment in the minority and renter sample permit a clear cut decision for adjustment? (Undercount Steering Committee Report, 1991, CAPE Report, 1992)*

Response: We will have a better idea when work on our estimator and poststratification is finalized. This work is premised on the 1990 scenario.

II. Sample Design

Issue II.1: *What are the underlying objectives (i.e., comparable CVs?) reflected in the sample allocation to states? (Research on Sampling and Estimation in the Census: Issues and Priorities," October 1995, Mary H. Mulry.)*

Response: The A.C.E. national sample consists of three components: 1) the general sample, 2) the American Indian Reservation (AIR) sample, and 3) the small block cluster sample. The general sample allocation is proportional to total population per 1998 total population estimates with a minimum of 1800 housing units in each state and 3750 housing units in Hawaii. The AIR sample allocation is approximately proportional to the 1990 American Indian population on reservations. The number of housing units to be interviewed from the block sample is expected to be low nationally and, consequently, should not significantly impact state interviewing workloads.

We arrived at this allocation by simulating alternative sample designs and comparing simulated coefficients of variation for the 1990 poststrata design. The sample allocation attempts to satisfy several conflicting objectives:

- improved reliability expectations for minority estimates,
- improved reliability for all 1990 poststrata,
- comparable reliability of total population among states,
- minimum sample size in a state,
- comparable reliability of total population among congressional districts,
- flexible allocation to accommodate an undefined 2000 poststratification, and
- support separate estimates of Asians and Hawaiians.

The reliability expectations were simulated for the 1990 poststrata. Performance was analyzed relative to both the 1990 results and proportional allocation of sample to states.

Issue II.2: *Can adequate sample be provided to ensure some minimal level of direct state estimation to offset NAS concerns about heterogeneity? (National Research Council, NAS Panel, May 3, 1999 letter report on the A.C.E. to Dr. Prewitt.)*

Response: The A.C.E. sample is designed separately by state with the national sample allocated to states proportional to population with a minimum sample size of 1800 housing units. However, we will not produce direct state estimates for anything but evaluation purposes.

Issue II.3: *What are our accuracy expectations for various levels of geography, including congressional districts? At what geographic level is adjustment no longer more accurate? (Research on Sampling and Estimation in the Census: Issues and Priorities," October 1995, Mary H. Mulry.)*

Response: We are in the process of analyzing errors for various levels of geography but expect error levels comparable to 1990. In 1990 we did not look at census geographical units, e.g., tracts and blocks. We looked at differing population areas, e.g., less than 25K, greater than 100K. Overall numeric accuracy (getting the count closer to "truth") was improved by adjustment. We concluded that distributive accuracy (getting the allocations of population shares closer to "truth") was improved for all areas greater than 100K population. We concluded that in areas less than 100K, the results were inconclusive as to whether adjustment was more accurate or just comparable to the census.

Issue II.4: *In 1990, although we differentially sampled, we had high variances for minority poststrata. How are we ensuring acceptable levels for 2000? (See discussion of weight variations among demographic groups and influential observations below)*

Response: Differential sampling is being investigated to increase sample size for the historically undercounted groups. A good differential sampling plan should improve reliability of the smaller population subgroups while having minimal impact on other subgroups.

Issue II.5: *How will we control weight variations among demographic groups?*

Response: To reduce weight variation among demographic groups, demographic strata formed in a single state will be kept to a minimum, i.e., two for heterogeneous states (minority/non-minority clusters), and three for states based on both housing unit count differences and demographic groups (minority block clusters, non-minority inconsistent clusters, and all remaining clusters). As noted for the previous item, some over sampling will be employed to improve minority estimates and to reduce outliers.

Issue II.6: How are we planning to deal with "influential observations"? Do we have a definition for which observations are to be considered disproportionately influential? (Mulry Research Paper) (Note, this issue also relates to estimation, census and A.C.E. implementation, sections III, IV, and V.)

Response: We are dealing with this issue in two ways. First, we will conduct a block cluster review by comparing the December 1999 MAF and the A.C.E. listing housing units counts. However, the details for how much of a deviation should be considered disproportionately influential has not yet been determined. Second, we are addressing this issue in our design of our Targeted Extended Search.

III. Estimation Strategy

I. Poststratification

III.I.1 Issue: Should we use region or some other geographic variable in defining our poststrata?

Response: Whether to use region or some other variable in our stratification is currently being researched. The Population Division/Demographic Analysis staff do not believe that we should use "region" in defining our strata nor use it as a poststratification variable. Dave Word distributed September 16, 1999, "Notes on Stratification" at the 9/16/99 Stat Design meeting from a demographic perspective. He pointed out that except for large urban areas in the East, the coverage rates are not differential based on the 11 remaining urban region geographic breakouts. Instead, adding nonresponse variable and household relationship in the poststratification may explain some residual heterogeneity. The National Academy of Sciences (NAS) has suggested that we redefine "region."

Issue III.I.1: Will statistical models beyond the synthetic model, such as smoothing or raking, be used in production of the adjusted counts? (CAPE Report, 1992)

Response: Currently, no statistical models other than the synthetic are planned for production.

Issue III.I.2: Since poststrata can now cross state lines, will there be separate poststrata for each race group?

Response: Yes. However, depending on geography, Asian and Pacific Islanders poststrata may have to be combined in many states.

Issue III.I.3: Are mail response rates going to be used in poststratification to help align A.C.E. estimates of undercount with subnational demographic benchmarks? (Also a demographic analysis consistency issue.)

Response: Mail response rate is a potential poststratification variable in the 2000 design.

Issue III.I.4: Will the 2000 poststratification scheme reduce or at least maintain the amount of heterogeneity present from 1992 levels? (Mosbacher Decision Paper, CAPE Addendum, 1992)

Response: Some of this work is still in progress, but we have conducted quite a bit of analysis on improving poststratification over 1991 and 1992 designs. Although we do not expect that heterogeneity levels will deviate substantially from earlier levels, we may get some improvement over 1992. However, our expectations are contingent on 1990 assumptions holding firm. For example, if the quality of the MAF declines or improves substantially from 1990, we could experience increases or decreases in heterogeneity.

Issue III.I.5: Will the 2000 poststratification scheme reduce or at least maintain the amount of correlation bias present from 1992 levels? (Mosbacher Decision Paper, CAPE Report, 1992)

Response: Some of this work is still in progress, but we have conducted considerable research into improving poststratification over 1991 and 1992 designs. Specifically, we have been looking into whether differentiating a household type (i.e., has or hasn't strong ties to household) will improve capture probabilities of the hard-to-count. However, this work is still under review. Two differences from 1990 could affect the level of correlation bias but end up being offsetting. On one hand, if the way that we are treating movers (PES C rather than PES B in 1990) inherently has more correlation bias then it will increase. We won't know that until after we complete an evaluation of PES C. On the other hand, we have launched an extensive comprehensive promotion, advertising, and partnership campaign focusing on the very population groups that are subject to correlation bias. As a result, we may see decreases in bias because of this massive outreach effort. Consequently, we have no reason to believe that the level will substantially increase from 1990.

Issue III.I.6: How will we handle multiple race entries?

Response: We have a proposed plan for the pending stratification specifications. After a decision is made, a file with all possible race combinations cross classified with Hispanic/non-Hispanic and in/not in Indian Country will be created to specify the coding scheme.

II. Other Estimation Issues

Issue III.II.1: Are we getting a good measure of gross coverage errors? (GAO 1993 Report, "Research on Sampling and Estimation in the Census: Issues and Priorities," October 1995, Mary H. Mulry.)

Response: Although we focus most on net error, analysis of the P and E sample will provide some measures of gross error. Gross error is important because if too large, it will substantially increase both heterogeneity and variance of our estimates. We currently have no plan for producing an estimate of gross coverage errors intend to ensure such information is available, if needed.

Issue III.II.2: Will our design control for classification error?

Response: We are in the process of reanalyzing our dress rehearsal data to determine levels and possible controls for classification error for our production poststrata.

Issue III.II.3: In 1990, there were concerns about residual balancing error in the PES design. Will our design control for balancing error? Will our search area produce balanced P and E samples?

Response: Bob is answering

Issue III.II.4: How will we ensure that ratio estimation bias will be minimized?

Response: The larger A.C.E. sample sizes could generally reduce bias below 1990 levels, although for Blacks the 2000 outcome may be only roughly as good as 1990. Estimates of ratio bias will be monitored during production. As a contingency, collapsing can be implemented.

Issue III.II.5: What is the plan for handling overcounts now that we have dropped SNRFU, given we were going to distribute overcounts to the blocks proportionately to the imputed population? (ASA Paper, Waite and Hogan, 1998)

Response: We are going to remove overcounts just as we did in 1990--not proportionately to the imputed population.

IV. Census Conditions

Issue IV.1: How much missing data/geocoding errors in the census can we tolerate before CVs become questionable? (Estimation Analysis, 1998)

Response: An error model developed by Bob Fay indicated that projections on ICM performance from a variance perspective was premised on the assumption that error levels in the 2000 census would be comparable to 1990. Bob identified a number of design and other changes that could alter that assumption and subsequently quantified the relative effects of those changes. His analysis illustrated that any substantive increase in geocoding errors and/or a high number of close-out cases in nonresponse follow-up would increase state-level CVs substantially.

Issue IV.2: What will be the effect on variances of a reduced search area for the A.C.E.? (Estimation Analysis, 1998)

Response: This work is currently in progress.

Issue IV.3: Do high numbers of erroneous enumerations drive the adjustment areas with low duplication rates to get high adjustments? What are the effects of high EEs on adjustment and can we do anything about it? (Wachter analysis of 1991 adjustment)

Response: Bob is elaborating.
Just like with geocoding errors, problems arise with erroneous enumerations when they cluster. In clustered blocks when the adjustment factor is applied, error level is exacerbated.

Issue IV.4: What performance indicators should be monitored during the census phase that could affect the quality of our estimates? (Estimation Analysis, 1998)

Response: We have not yet begun fully examining this issue but are considering: reports of geocoding errors, mail response rates, close-out rates, last-resort rates, and missing data rates to mention a few.

V. A.C.E. Survey Implementation

Issue V.1: Can we achieve a 95% response rate with low differential to keep our bias levels acceptable? (Estimation Analysis, 1998)

Response: In 1990, we achieved a 98.6% response rate for the PES, which was the initial goal for 2000. However, in August 18, 1997, the Field Division sent a memo that stated that this goal is not reasonable. They believe that an overall response rate of 95% with local variation is a more reasonable expectation. After analyzing a couple of options for boosting response rates, we have concluded that a 95% national response rate is acceptable provided there is not too much variation at the LCO level. We would be concerned if any LCO has a response rate much lower than 90% even if the national average is 95% or more. However, imputation bias, negligible in 1990, will increase with a 95% response rate.

Issue V.2: *Is our plan to handle missing data on item responses, and whole households robust? What levels of missing data in the A.C.E. become problematic for adjustment? (Mosbacher Decision Paper, Wachter analysis of 1991 adjustment, Estimation Analysis, 1998)*

Response: As far as robustness, we are in the process (in concert with the NAS) of examining our imputation model. We have not yet determined how much missing data becomes problematic. M.I,. Pat C.

Issue V.3: *Will the planned system for matching reduce or at least maintain the amount of matching error in the A.C.E.? (Mosbacher Decision Paper, Wachter analysis of 1991 adjustment)*

Response: The planned system for 2000 will reduce the matching error below 1990 levels. The 1990 system was clerical using paper and the results were keyed into the software. The software for 2000 is paperless. There are edits built into this software that will make matching error virtually disappear. There are many checks during the matching that only allow them to do the correct coding. The clerks will be more well trained in 2000 also to handle the matching situations. This software automates the searching that was manual using printouts in 1990.

There will be quality assurance to guarantee a quality product. We have been training the technicians who do the quality assurance since September. They will be ready for the matching next year and will do it accurately. The design for 2000 is matching only people who lived in the cluster to census enumerations in the cluster.

Issue V.4: *What performance indicators should be monitored during the A.C.E. survey? (CAPE Report, 1992)*

Response: We have not yet fully examined this issue but candidates to monitor could include: response rates, match status tallies, TBD

VI. Comparisons to Demographic Analysis/Other Evaluation Issues

Issue VI.1: *How will we deal with inconsistencies between demographic analysis and DSE estimates?*

Response: During production, we will be monitoring how our estimates compare with demographic groups at varying geographic levels. This will help us to identify areas that may need closer examination. At the end of the process, if differences exist, it will be too late for us to do anything more than report them.

Issue VI.2: How will we measure correlation bias in 2000 and how will we allocate it to sub-national levels?

Response: We plan to measure correlation bias for research only not to adjust production estimates. To reduce correlation bias in the estimates that we will use as "truth", the plan is to combine DSEs from these estimates with results from DA, in particular, with DA sex ratios using a two-group combining approach. How do we allocate it to sub-national levels--same as in 1991 and 1992? Bob, I think Bill has a way to do this for females as well??

Issue VI.3: What is our plan for handling negative values in the 4th cell? (CAPE 1992, Mulry Research Paper 1995, and Wachter's analysis of 1991 adjustment)

Response: Bob is answering

Issue VI.4: How will we measure A.C. E. bias components in 2000?

Response: PRED with Bruce Spencer is developing a total error model for use in A. C. E. evaluations. The evaluation results will feed into the total error model for use in loss function analyses. The total error model is premised on the 1991 model.

Issue VI.5: What will be the effect of our decision to use PES C on bias levels compared to 1992? (Cape Report, 1992, Estimation Analysis)

Response: We will not know this unless our evaluation includes a PES B type comparison. This decision is still pending. The Evaluation Follow-Up interview is being designed to collect information on where inmovers were on census day in case it is decided to do such an evaluation. At this point, no money (i.e., staff) has been allocated to do such a comparison. Funding and staff would be required to do the geocoding to census day address, to develop the required matching system, to conduct the matching, and to analyze PES B v PES C. This work could possibly be done in FY 2002, if money and staff were available. We will not be able to follow-up on inmovers (as in 1990) that we could not find at their census day residence.

Issue VI.6: Will we have situations as in 1990 where we had a large number of overcounted blocks (about 2000) that were adjusted for undercount?

Response: Yes. The phenomenon of 1990 is probably due to geocoding errors and erroneous enumerations in the census and to synthetic estimation. When the adjustment factors at the poststrata level are pushed downward, some of the blocks were over- versus undercounted. It is simply an offshoot of the methodology and can best be obviated by preventing or correcting geocoding errors. Further, the problem may be overstated by including small blocks highly susceptible to counting errors.

CONCLUSIONS AND RECOMMENDATIONS

TBD

2.1 Introduction

Whether one is dealing with a census, a survey, or an administrative record system, the first question that can be asked is whether any members of the population of interest are systematically omitted or underrepresented. Are units omitted (e.g., people, houses, businesses, farms) about which the user would like to gather information, thus creating an error of undercoverage? Are the statistics a produce of a data set that includes out-of-scope units or units included twice, creating an error of overcoverage? This chapter discusses coverage errors, how they arise and the methods used to address them.

The ultimate goal of any data collection is to make some statement about the larger population that is of interest to the research goals. In sample surveys, measurements are taken on the sample to learn about the characteristics of the full population. Coverage error arises when a survey fails to include some elements of the population of interest. Coverage error occurs early in the survey process, usually at the stage when the materials used to identify persons to measure are compiled or drafted.

To discuss coverage error in a meaningful way, one must first define and discuss the population of interest to the researcher. The following definitions draw heavily from Groves (1989). The **population of interest** is often referred to at the population of inference. The **population of inference** is defined as the set of persons to be studied, with explicit delimitations with regard to the time interval over which research interest is focused. The population can be finite or infinite. Conceptually, coverage error becomes meaningful only after it is related to the data user's population of interest. Unless the data user can define the population he or she is interested in, one cannot measure how well the population included in the statistical system corresponds to the user's needs. Defining a population is no simple undertaking. Familiar concepts such as housing unit, business, or college students are, in fact, quite imprecise. Does a rented room with a separate entrance, bath, but no kitchen constitute a housing unit? When does a moneymaking hobby become a business? Can a post-high school student at a trade school be considered a college student? If not, how does one treat a student taking the same trade classes at a community college?

The population of interest is a function of one's research interests and can vary from data user to data user or from researcher to researcher. The target population, however, is the population for which data are collected and inferences (the estimates) made. The survey designer defines the target population while the survey is still in the planning stages. The user might want to know about the entire population of the United States (perhaps more narrowly defined as those who spent 90 of the last 120 days in the United States). To produce estimates about the entire population of the U.S., the survey designer may decide to exclude the population of remote rural Alaska, the homeless, or those who are currently abroad. Collecting data on rural Alaska, the homeless, or those who are abroad would be very costly and these groups entail so few people that their exclusion would not affect the estimates enough to justify the cost and effort of collecting the data. Similar examples are a business survey that excludes sole-proprietor businesses in the service sector, or a telephone survey that excludes unlisted numbers. Deliberate exclusions do not constitute coverage error.

The target population is related to the inference population, but is different from it. The **target population** is simply the set of persons that will be studied. This population is finite. Differences between the population of interest and the target population are important and should be discussed and reported in the survey documentation, but these are not defined as coverage errors. For example, the Current Population Survey (CPS), a household survey, explicitly excludes individuals who are institutionalized. Thus, the CPS's deliberate omission of individuals who are institutionalized is not a coverage error, nor can it be considered as any type of nonsampling error. The exclusion of the institutionalized population is probably a negligible issue for researchers wishing to estimate unemployment rates (which are based on individuals in the labor force). Presumably, there are relatively few individuals who are both in the labor force and in the institutionalized population. On the other hand, if the CPS was used to estimate the incidence of disabilities among adults, the exclusion of institutionalized individuals would lead to a serious underestimate of the population of interest.

The **frame population** is the set of persons for whom some enumeration can be made prior to the selection of the sample. In simple cases, the frame population is available on an already existing list, such as, a list of geographic areas, persons, addresses, or telephone numbers. When this is so, the list of persons, addresses, or telephone numbers is called the **sampling frame.**

A more formal definition of the **sampling frame** is found in Wright and Tsao (1983, p. 26): "The materials or devices which delimit, identify, and allow access to the elements of the target population. In a sample survey, the units of the frame are the units to which the sampling scheme is applied. The frame also includes any auxiliary information (measures of size, demographic information) that is used for (1) special sampling techniques, such as, stratification and probability proportional to size selections; or for (2) special estimation techniques, such as ratio or regression estimation."

The frame is more than a list, although it usually includes a list. The frame consists of a set of procedures to associate the actual population units with each item on the list. The population covered by the frame is called the frame population or sometime the survey population

Another important concept when discussing coverage is the **survey population** which is defined as the set of persons selected as potential respondents for the survey. These persons are accessible to the interviewers and are physically and mentally capable of providing answers to the survey questions.

Based on the definitions above, we can say that **coverage error** is the difference between the statistics calculated on the frame population and the identical statistics calculated on the target population. Coverage error arises from omissions, erroneous inclusions, and duplicates in the sampling frame. **Omissions** reflect the fact that some units in the target population have been omitted from the frame. Omission from the frame means that these units have no chance of being included in the survey. **Erroneous inclusions** reflect the fact that some units not belonging to the target population have been included in the frame. **Duplicates** are defined as target population units that appear in the sampling frame more than once. Omissions give rise to **undercoverage** and erroneous inclusions give rise to **overcoverage**. Kish (1965, p. 529) provides the traditional definition of coverage error that focuses on one aspect of coverage error, that is, **noncoverage**.

> [N]oncoverage denotes failure to include some units, or entire sections, of the defined survey population in the actual operational sampling frame. Because of the actual (though unplanned and usually unknown) zero probability of selection for these units, they are in effect excluded from the survey results. We do not refer here to any deliberate and explicit exclusion of sections of a large population from the survey population.

Note that here coverage is defined in terms of the operational sampling frame, emphasizing the fact that the frame is often defined by a process rather than a simple list. Later on the same page Kish notes that groups of units are included or excluded to varying degrees, whereas individual units are either included or excluded. For example, a group can be covered to 50, 80, or 90 percent, rather than all or nothing. The same discussion also applies to overcoverage, that is, the inclusion of out-of-scope units or duplicate units.

The main reason coverage issues are of concern is that units omitted or erroneously included may be different or distinctive in some respect from those included in the survey. If this is the case, the resulting statistics will be biased. It is also important to note that coverage error is not a function of sampling and is a source of error in censuses.

2.1.1 Coverage issues and census methodology

Since much of the literature on coverage error grew out of census methodology, it is now useful to review censuses and their terminology. Omitting a unit from a census causes a **gross undercount**. A gross undercount is defined as the unintentional exclusion of a member of the target population, without taking into consideration that over- and undercounted units may (or may not) eventually cancel each other out. Analogously, the unintentional inclusion of an out-of-scope or duplicate unit causes a gross **overcount**. The difference between the true population and the census tally for a given group constitutes the net undercount. A negative net undercount is, of course, a net overcount. The net undercount for a group is caused by both coverage error (gross omissions and gross erroneous inclusions) and classification error (gross misclassification out of the group and gross misclassification into the group). At the national level, all misclassifications between groups cancel out and so net undercount equals net coverage error. For subpopulations, classification error may predominate. A classic example is age groups where misclassification is an important element in net undercount. These ideas are easily generalized to an administrative record frame or sample survey.

The simplest illustration of coverage error is represented by a census of all units of analysis. For example, we wish to conduct a census of all businesses in a small town. Once we have clearly defined what we mean by a business, the next step is to construct a frame. We might construct the frame using telephone directories and city directories, and also by canvassing the town for businesses.

Since this is a census, we wish to include each in-scope unit with a probability of one. Each business has an unknown chance of being included on the frame. For large and visible businesses, this chance is very near one (but mistakes do happen). For smaller home-based businesses, the chance may be near zero, but never precisely zero. For example, an informant could disclose the existence of a very small home-based business, so its probability can never be exactly zero. As with any stochastic process, we can discuss both the underlying probabilities and the outcomes of any one replication.

Now consider a more complicated situation. We wish to select a sample of people in a town. Beginning with a set of maps, we select a sample of areas (blocks). We then send field staff into these sample blocks. They are instructed to list all housing units. In the office we select a sub-sample of the housing units the field staff listed. Interviewers return to the units and conduct interviews. As the first part of the interview, the interviewer is to list all people living in the sample housing unit.

Clearly, no actual list of all the people in the town is ever constructed. The entire listing and sampling procedure constitute the operational sampling frame described by Kish. In other words, the frame is defined by a procedure, not a list; but nonetheless the procedure per se entails probabilities of omission or erroneous inclusion.

Section 2.1.2 How coverage errors occur

How might coverage errors occur? Keeping to our example, we might fail to include a block on the list. Or, the field staff may not list all units in the sampled blocks, causing gross undercoverage. Or, field staff might stray outside the boundaries of the sampled blocks and include units in nonsampled blocks. Interviewers may go to the wrong (out-of-sample) housing unit and interview there, causing one household to be incorrectly omitted and another to be incorrectly included. Staff may go to the right housing unit and fail to list all people living there, or list people not living there according to survey definition.

In complex surveys, no complete list of sample units is ever constructed. Nevertheless, one can discuss the coverage of the "operational frame" in terms of probabilities and expected values. By considering the outcomes of many (hypothetical) repetitions of the frame construction procedure, coverage bias can be discussed in terms of expected values.

2.1.3 Differentiating coverage errors from errors of nonresponse

The distinction between nonobservation due to coverage errors and nonobservation due to noninterviews is not always clear. However, it is a useful distinction and can be clarified somewhat by thinking about what is meant by nonobservation. Coverage error is totally nonobservable, that is, it leaves no trace of its own existence. This is equally true of both undercoverage and overcoverage. Nonresponse error, on the other hand, generates information about itself and in that way leaves a record of its existence. Nonresponse errors can be discussed and analyzed, at least in part, by using data gathered by the survey itself. This "self-generated" evidence never is present with coverage errors. An example may help clarify the difference. Assume that according to survey definitions a structure should be listed as a housing unit, but that an interviewer is unable to decide whether it is a housing unit or not. If the interviewer does not list the structure, he/she has introduced a coverage error. If he/she lists the structure, but is till uncertain about its classification and marks it "unclassified," he/she has introduced a nonresponse case.

To analyze coverage errors, it is necessary to assume that adjustments have already been made for nonresponse. In the case given above, we assume that the imputation mechanism creates a set of person records for the nonresponse households and these households will have the same coverage as if they had actually responded.

2.2 Measuring Coverage Errors

Since coverage errors never leave any apparent indication of their existence, they can be measured only by reference to an outside source. Essentially, there are two methods of measuring coverage errors: aggregate comparisons to other sources and case-by-case matching.[1]

2.2.1 Comparisons to independent sources

One can sometimes find or construct a better aggregate estimate of the frame population than is available from the survey. For example, it is often possible to compare the age, race and sex distribution of the survey population to that of the census, demographic projections made from the census, or estimates based on analytic techniques. Such comparisons must be made taking into consideration the errors in both the survey being evaluated and the estimates of the reference population. Often differences too large to be attributable to sampling errors are found, and other sources of error, such as coverage error, must be considered. Of course, the census itself does not have complete coverage, and this should be taken into consideration in the evaluation.

When a better aggregate estimate of the frame population is available, a common method of comparing the two estimates is the coverage ratio. The coverage ratio is calculated as the estimate from the survey divided by the better aggregate estimate (i.e., an independent population control total) where the survey estimate is first adjusted for nonresponse. The ratio as a measure of coverage has several drawbacks. First, a superior estimate that can serve as a population control must be available or at least constructible, which is not always possible. Secondly, these comparisons usually yield only information about net differences in the counts of various groups. For example, one might find that the survey counted fewer housing units than the outside source indicates are actually present. Aggregate analysis does not give information about the sizes of the gross errors. We might have incorrectly given many units too low a chance of observation, while at the same time, our procedures may incorrectly increase the chance of including others. These errors may "net out" for the categories where independent estimates are available, but still create important bias for other variables of interest. Using the above example from business surveys, the estimated number of businesses may be approximately correct; however, if there is overcoverage of large businesses and undercoverage of small businesses, there may be considerable bias in the estimate of average sales. Conversely, it is possible that

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[1] Occasionally, the coverage errors will present indirect internal evidence. For example, in demographic surveys, the age and sex structure or the distribution of households by size might lean one to conclude that certain groups had been omitted. The evidence might equally be the result of classification error. In any case, these specialized demographic techniques will not be discussed here. The interested reader is directed to Shryock and Siegel (1975).

there is considerable underrepresentation or overrepresentation without incurring large bias in the estimates of interest. For example, if one is interested in estimating total sales within an area, undercoverage of small businesses results in much less bias than undercoverage of large businesses.

The aggregate method is probably most informative when it is possible to identify subgroups which are likely to have different undercoverage rates and estimates of the relative undercoverage for the different groups can be computed. For example, the Survey of Research Scientists (SRS) integrated file of individuals with science and engineering degrees is known to have a more severe undercoverage problem for foreign-educated scientists and engineers than for U.S.- educated scientists and engineers. Decennial census figures can be used to estimate approximate percentages of all foreign-educated scientists and engineers. Comparison of the decennial percentage with the percentage in the SRS file provides an estimate of the amount of bias introduced by this measure. Steps can then be taken to adjust (formally or informally) for this bias.

The final difficulty is that the difference between the survey and the population controls may be due to factors other than coverage. This problem pertains to all measures based on residuals, where residuals are the differences between the survey estimates and the controls. For example, suppose a survey estimates the number of people with a CV of 5 percent and has a net undercount of 10 percent. If the control population is fixed and not subject to sampling error, the residual will have the same sampling error as the total. In this case, one would be able to reject the hypothesis of perfect coverage only about half of the time.

2.2.2 Case-by-case matching and dual system estimation

A second approach is based on case-by-case matching. Often, an alternative list of units exists or can be constructed. We can classify all units in the population as either present in our census/survey/record system or not present. The same classification is conducted on the alternative list. So we can cross classify all units as:

	Alternative Frame		
	In	Out	Total
Our Frame			
In	N[11]	N[12]	N[1*]
Out	N[21]	N[22]	N[2*]
Total	N[*1]	N[*2]	N[**]

Where the asterisk indicates summation over that column or row. Clearly,

$$N[**] = N[11] + N[12] + N[21] + N[22]$$

or, the total population equals the population on both lists, the population on only our list, the population only on the alternative list, and the population not on either list.

The population not on either list is, of course, not observable. However, one can estimate it if the two lists are, approximately, independent. Essentially, this means that the probability of being included in one list does not depend on the probability of being included on the other. (See Wolter, 1986 for a more precise mathematical description).

Under conditions of independence, we can estimate

$$N[1*] / N[**] = N[11] / N[*1]$$

which is the coverage ratio of our frame, using the alternative list as a control. We can also estimate the total population simply by rewriting this equation

$$N[**] = N[1*] N[*1] / N[11]$$

which is algebraically the same as

$$N[**] = N[11] + N[12] + N[21] + N[12] N[21] / N[11]]$$

This estimator has a long history; it has been used in studies of wildlife where it is known as the Peterson estimator (Peterson, 1896). In human populations it is sometimes known as the Chandasekar-Deming estimator after an early application to birth registration completeness (Chandrasekar and Deming, 1949). It is most often called the dual system estimator (DSE), and is often used to estimate census coverage (see, for example, Marks, 1978).

Note that the concept of the DSE rests on the assumption that all units in the population have a probability of being covered. First, we assume that the event of being included in one system does not change the probability of being included in the other: this is causal independence. It is the responsibility of survey management to take administrate steps to ensure that this assumption holds.

Second, we assume that all units within a frame have the same probability of being included. This probability may differ for each frame, so long as it is constant within one frame. Since this condition rarely holds, even approximately, for all units in a survey or record system, the DSE is usually calculated on subpopulations where the condition is more likely to hold. For example, while it is unlikely that all farms are included with equal probability, it might be reasonable to assume that all small farms in the South are included with equal probability. In the DSE literature, these separate estimation cells are called post-strata.

The DSE data must be modified to remove duplicates. Essentially, one must remember that the numbers in the table above are the counts of true, unique, and correct units included in each system. The literature on the mechanics of measuring coverage using the DSE is large and readily available. See for example, Marks, Seltzer, and Krotki (1977) and Hogan (1992, 1993).

There are other ways to evaluate coverage than using the DSE. Often post-enumeration surveys try to determine the total population by actually finding all people who were missed in the census. That is, they ignore the N[22] cell (i.e., the units not captured in either frame) and instead estimate

$$N[**] = N[11] + N[12] + N[21]$$

The original Post-Enumeration Survey (PES), conducted in the United States after the 1950 census, used this approach. Several other countries have also used it.

This approach seldom works except perhaps when census coverage error is so low that there are few omissions to find. The problem is, of course, that it is even harder to conduct a perfect enumeration (even on a sample basis) several months after the reference date than to do the original count. The second survey often misses more people than the first. Nevertheless, trying to find missed units after the original enumeration can be useful, especially when the characteristics of the missed units can be ascertained. (See Section 2.4.1).

Post-enumeration surveys have not been limited to human populations. There have been PESs to measure the coverage of other units, such as, schools, housing, and farms. The National Center for Education Statistics (NCES) used a DSE to measure the 1993 Private School Universe. The original frame was constructed using a list frame of private schools. It contained 24,067 schools. In a subsequent area frame, 21,613 schools were captured of which 19,587 were also in the original frame. So the estimated total number of schools would then be

$$\text{Total} = (24{,}067)\text{x}(21{,}613)/\ 19{,}587 = 26{,}556$$

Since their actual frame consisted of the two frames combined

$$24{,}067 + (21{,}613 - 19{,}587) = 26{,}093$$

So that the coverage of their combined frame is estimated as:

Coverage (%) = (26,093/26,556)x 100 = 98.3%

2.2.3 Other approaches to coverage measurement

A superior estimate may sometimes be constructed on a subsample of cases, where more accurate, and presumably more costly, methods are employed. Groves (1989) presents the following example:

> One example of a special coverage check study comes from the National Survey of Black Americans, an area frame household survey of blacks conducted by the Survey Research Center. Since black households form roughly 10 percent of all households in the United States, the surveying of that subgroup entails large screening costs, especially in areas where few blacks were found to reside by the last census. To reduce the costs of frame development in those areas, instead of screening the residents at each listed unit to learn their races, screening was done only at a subset of houses. In addition to inquiring about the race of the residents of the house visited, however, the interviewer asked whether any blacks lived in the area described by a map of the sample block (or other sample area chosen). If none of the visited houses identified a black household in the sample area, no further visits were made in the area. If a black family were identified, interviews were attempted at those units. This method relies on sample area informants to provide frame information on sets of potential sample housing units and thus saves large amounts of screening costs.
>
> As a check on this method, interviewers were asked to visit all households in a subset of the blocks, both to ask the screening questions of each visited household and to use each as an informant about the residence of black persons in the sample area. Since the subset of blocks on which both methods were used was a probability sample of all areas, the check yielded estimates both of the rate of noncoverage of the cheaper method, and also of the characteristics of the black persons who would not be covered by the cheaper method. This is an example of using an expensive method in a subsample to evaluate a cheaper frame construction method used throughout the survey.

Another example comes from a telephone survey that uses random-digit-dialing (RDD). RDD, like all telephone surveys, excludes households without telephones or with interrupted service. In one type of RDD design, telephone numbers in clusters of telephone numbers ("100-banks") with few residential numbers are also excluded. By analyzing results from a personal (face-to-face) interview survey, it is sometimes possible to get information on the characteristics of households missing from the RDD frame.

Giesbrecht (1996) and Giesbrecht, Kulp and Starer (1996) conducted an analysis using CPS data. By matching the telephone numbers collected in the CPS to the RDD frame, they were able to determine the number and characteristics of households not covered by various RDD sampling plans.

2.3 Assessing the Effect of Coverage Errors on Study Estimates

The literature on coverage measurement tends to focus on estimating the number of missing or erroneous units. Less work has been devoted to measuring the effect missing units have on survey results, for example, statistics of unemployment or total sales. Thus, it is not always obvious how much of the bias and the total survey error emanate from coverage errors.

Estimating bias resulting from coverage errors is a fairly difficult task. It is unlikely that a useful independent source exists that permits estimation of coverage bias. Consider a survey that measures retail sales by surveying stores during a given month in a given city. One might find a recent economic census that could tell approximately the number of stores. However, if there was a reliable independent source for the monthly retail sales, there would be little point in running the survey. The best that one can hope is that an annual survey might produce annual sales data that can be compared to the sum of the monthly estimates. (See Section 2.4)

Matching studies are useful here, but entail their own set of problems. The matching studies will have identified a number (N[21]) of units in the population that were not included in the survey frame. However, if the alternate source was an administrative record system or other pre-existing data file, it is not likely to contain the same information that the survey would have collected on the missed units. For example, a survey on the health conditions of one month old babies might be matched against the birth records. The birth records of the missing babies might provide useful information about weight at birth, sex or race, but will not include information about the health at one month.

When the second frame is under the direct control of the survey manager it is possible to again ask many or all of the important items. This information allows us to say something about the characteristics of the missed units included in the second frame (N[21]). However, it tells us nothing directly about the units estimated to be missed by both frames (N[22]). One way of evaluating the units in N[22} is to assume that the missed-by-both units have the same characteristics as the missed-by-one units. If a two-way match has been performed, it is possible to compare the characteristic of the N[12] units with those of the N[21] units. If these are quite similar, one would be comfortable in assuming that the N[22] units are similar as well.

The bias introduced by missing units will depend on several things. Lessler and Kalsbeck (1992, pp. 58-61) analyze the problem in the following manner:

Define

$\overline{Y}_o$ = Mean of omitted population

$\overline{Y}_a$ = Mean of frame population

N = Number in target population

N_o = Number omitted (=N[12])

Let r be the ratio of population mean of the omitted units $\overline{Y}_o$ to the mean of the frame population $\overline{Y}_a$.

$$r = \frac{\overline{Y}_o}{\overline{Y}_a}$$

W_o is the proportion of units omitted

$$W_o = \frac{N_o}{N}$$

The relative bias for estimating a population total is

$$\frac{-W_o r}{rW_o + (1 - W_o)}$$

This will be small whenever either r or W_o is small. For example, mobile food carts are often omitted in surveys of retail trade. Although they may represent a measurable proportion of retail outlets W_o, their average sales $\overline{Y}_o$ are much smaller than for shops and stores $\overline{Y}_a$, so r is relatively small. The coverage bias is ignorable.

When estimating means the situation differs. Here the relative bias may be written as

$$\frac{W_o(1 - r)}{(1 - W_o) + rW_o}$$

Obviously, there is no bias if the mean of the omitted units is the same as the mean of the included units $(r = 1)$. So long as r is close to unity and W_o is small, the relative bias on the mean is ignorable.

Indeed to have a large effect on the estimated population means, the population not covered by the survey must be large and quite different from the covered population. For example, assume that the survey covers only 90 percent of the population and estimates that 5 percent of the population is unemployed, infected, smokes, or has some other characteristics. If the proportion possessing this characteristic among those missed is three times greater (i.e., 15%) then the true proportion would be $.9\,(.05) + .1\,(.15) = .06$ or 6 percent rather than the 5 percent estimated from the frame population.

Depending on the character of interest, and the level of other errors, this difference might be ignorable.

An example of measuring the effect of coverage error comes from the National Household Education Survey (NHES). The NHES is a data collection system of the National Center for Education Statistics (NCES). The NHES is a random-digit-dial telephone survey and only included persons who lived in households with telephones. Approximately 6 percent of all persons live in households without telephones, according to data from the March 1992 CPS.[2] The CPS does not systematically exclude nontelephone households. The percentage of persons who lives in households without telephones varies by characteristics of the population considered. For example, while 95 percent of all adults live in telephone households, only 87 percent of black adults and 88 percent of Hispanic adults live in telephone households. (See U.S. Department of Education, National Center for Education Statistics Working Paper No. 96-29, 1996 from which this material is derived.)

An important focus was on statistics for the population 0 to 2 years old who were sampled as part of the Early Childhood Program Participation (ECPP). Supplements to the October 1992 CPS were used to examine the extent of the differences in the characteristics of persons in the telephone households and the nontelephone households. The items included in the supplement were limited, containing items about care arrangements and disabilities. More information was gathered on adults.

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[2] Which, of course, is subject to undercoverage problems of its own, See 2.6.1 below.

By tabulating the characteristics of the telephone and nontelephone households from the CPS, the NCES was able to assess the bias due to excluding non-telephone households in the NHES and ECPP. They concluded:

> The analysis of undercoverage bias shows that the coverage biases for estimates of adult characteristics are not very large, while for 0- to 2-year-olds the biases are somewhat larger, but still relatively small. The undercoverage bias for subgroups ... may be more problematic. No specific rule can handle all the subgroups that may be considered by analysts of the NHES:95, but some guidelines are possible. When dealing with a small subgroup that is likely to be differentially covered, analysts need to account for both sampling errors and nonsampling errors. For example, estimates from the NHES for a poorly-covered subgroup such as black children might be approached differently than analysis of all children. Therefore, it is recommended that estimated differences between poorly-covered and well-covered groups (such as black and nonblack children) be considered substantively important only if the differences are larger than both the sampling error and potential coverage bias error (NCES, 1996, pp.10-11).

This report delineates what we consider good practice in this area.

2.4 Correcting for Coverage Error

There are essentially two approaches to overcome coverage error: (1) improve the frame before data are collected and (2) adjust the data after they are collected. The most straight forward approach is to take steps to improve the survey frame. Occasionally this can be done by putting more time, money or staff into frame development. For example, one might add a quality control step to address listing. Or, one might decide to include telephone banks with only one listed residential number in a survey that had previously excluded these telephone numbers and households. Such improvements are survey specific. They are seldom inexpensive, but nonetheless can prove cost effective (Lessler and Kalsbeek, 1992).

2.4.1 Dual Frame Approach

A related approach is to use two or more complimentary sampling frames, often called the dual frame approach. For example, the main sampling frame for the Current Population Survey (CPS) is the list of addresses enumerated in the previous census. This frame is reasonably complete and allows the sample design to use very small sampling clusters, four housing units, at reasonable cost.

Of course this primary CPS frame excludes all housing units constructed since the previous census. Since one would expect new construction to be closely related to economic growth, and the unemployment rate, an aging census frame could contain serious omissions. Therefore, the census list frame is supplemented by a frame based on building permits issued after the census. Since the frames can be easily unduplicated, they form the basis of improved estimation.

Often, a relatively expensive area sample is used to supplement a telephone or list sample. For this approach to work, one must have access to an affordable and accurate way to unduplicate the population covered by each frame.

Consider again the Private School Universe Survey (PSS) discussed above. A list frame is constructed from multiple sources, with the intent to include all private schools. A sample can be drawn from this list. In addition, a complete area frame is constructed and an area sample selected. Private schools in the area sample can be listed. Those already appearing on the list frame can then be discarded, and only the previously unlisted schools interviewed. In this way, coverage has been improved.

The area frame accounted for 7.8 percent of the estimated total number of private schools. The addition was much higher for some private school subgroups. For example, the area sample accounted for 15.3 percent of unaffiliated religious schools and 20.5 percent of Special Emphasis Schools (U.S. Department of Education, 1996a).

A cautionary lesson can be learned from the Census Bureau Monthly Retail Trade Survey. This survey began principally as an area sample supplemented by a list sample of larger retailers. By the 1990s, it had evolved to principally a list sample frame from tax and employer records. New retailers and nonemployer retailers were sampled from the area sample. These accounted for less than 5 percent of total retail trade. The cases sampled in the area sample could easily be checked against the whole list based on this Employer Identification Number (EIN) to see whether they had ever been subjected to sampling. If they had been, they were discarded. Units not subjected to sampling in this list sample were continued in the area sample.

The area sample was discontinued in the mid-1990s. It was extremely expensive to maintain. Since the area sample interviewers seldom encountered actual respondents, they had little practice conducting the full interview and often introduced response errors which partially off set the quality gains from the improvement in coverage.

An important consideration in the decision to eliminate the area sample was the variance added through the area sample. Since the area sample per unit costs were high, the sampling rates were relatively low and the weights for tenants from the area sample were relatively large. This will often be the case since the most cost effective frame will usually be chosen as the primary frame. The large weights resulted in a substantial increase in the variance of the estimates. So, the increased variance from the area sample offset any quality gain in terms of bias reduction from improved coverage.

Another factor in the decision was the ability to use estimation techniques that reduced the coverage bias. This is discussed in the next section. Balancing cost, response errors and variance against improved coverage, the Census Bureau discontinued the use of dual frame (Konschnick, 1994).

2.4.2 Post-stratification

In surveys in which there are auxiliary variables that can be used to post-stratify, coverage bias may be reduced. The Census Bureau was able to discontinue the area sample in the Monthly Retail Trade Survey because it had access to survey controls from the annual retail trade survey and the Census of Retail Trade. It thus used post-stratification to help account for the missing retail establishments. Post-stratification is defined as a process by which all units in the sample are classified into groups or estimation cells. This classification usually takes place after sample selection and data collection, thus the appellation post-stratification.

For each post-strata, we estimate the per-unit value. For example, in a demographic survey we could post-stratify into males and females, blacks, whites, and Hispanic. We could measure the unemployment rate for each post-strata. Because of coverage errors, some groups (e.g. black males) may be underrepresented in our sample. However, if we know the proportion of each post-strata in the population as a whole, we can apply those rates from our survey to the proportions from the population to produce a more accurate estimate. The estimate is said to have been corrected, adjusted, or controlled to population totals. The population information is usually described as population controls or control totals. Often these controls are based on a recent census.

Post-stratification, obviously, works well when the noncovered population is similar to the covered population in the post-stratum. Thus to be effective, the post-stratification variables must be correlated with the variables of interest. They must also be well measured in the survey and the control totals must be available for the population as a whole. Race and sex are obviously correlated with unemployment, but so is geography, age, etc.

Because of these complexities, survey results are sometimes controlled in several different dimensions in a process known as raking or iterative proportional fitting. The survey totals may first be forced to agree with population estimates by race, and then by city, etc.

Since it operates on survey totals, post-stratification can simultaneously control for coverage, nonresponse, and sampling errors. Clearly, post-stratification can be somewhat of a "fix."5 This is why coverage ratios should be reported for the "un-post-stratification" survey results.

2.5 Reporting Coverage Error

During the last ten years, coverage as a source of error in censuses and surveys has received considerable attention. The U.S. Decennial Census and its undercount of minorities has heightened awareness of this source of error. Federal Committee on Statistical Methodology working papers on the quality of establishment data (working paper 15) and on survey coverage itself (working paper 17) have described and discussed the issue in some detail and have contributed to the awareness. Despite the continuing and strong interest in this important topic, the reporting of coverage as a source of error remains inconsistent and incomplete.

In short format government reports (ten pages or less), such as **Census Briefs, Issue Briefs,** and **Statistics in Brief,** coverage error is mentioned infrequently. McMillen and Brady (1999) report that authors of short format reports cited coverage rates or mentioned coverage as a potential source of error in only 9 percent of the reports reviewed. The reports studied are short and have a page limit constraint, thus there is little opportunity to document this error source.

Analytic reports, reports that are the primary method for releasing results from a one-time survey or an on-going series of surveys, however, provide substantially more complete reporting of substantive results, either in narrative form, displayed in tables or graphical formats or some combination, and usually have no page constraints. These reports may provide more sophisticated analyses; thus, the target audience for this type of report is not the general public, but rather a somewhat more knowledgeable group of users. The reports typically dedicate a section or appendix to "technical notes" or a "source and accuracy statement" that contain information on the nature and extent of error sources as well as limitations of the analysis. In their review of analytic publications, Atkinson et al. (1999) report that only 49 percent of the publications they studied specifically mentioned coverage error as a possible source of nonsampling error, and only 16 percent provided an estimated coverage rate. Information about the universe and the sampling frame were more commonly reported.

The cost and complexity of measuring coverage error results in substantial difficulty in reporting quantitative evidence on this source of survey error. Typically, coverage studies are reported as a technical report or a special study where detailed tables provide estimates of undercounts on many characteristics, such as in the 1992 Census of Agriculture (U.S. Bureau of the Census, 1996). Regrettably, these studies are reported late after the initial results are released, and, therefore, are ignored by policy-makers who use the survey data.

Users' Guides and Quality Profiles, where substantial information can be summarized, are designed to help the user analyze and understand the data's limitations. By design , these types of reports are the best vehicles for communicating such information.. the Users' Guide for the Survey of Income and Program Participation (U. S. Bureau of the Census, 1998) provides useful summaries of the differential undercoverage of demographic subgroups in the SIPP; furthermore, details about the differential undercoverage can be found in the SIPP Quality Profile (U. S. Bureau of the Census, 1998). Ultimately, electronic linkage of the detailed technical information to the analytic reports will be become routine (Giesbrecht et al., 1999).

The nature of the publication (short-format or analytic) and survey (one-time versus continuing) plays a significant role in determining what and how much an analyst reports about this source of error. The studies conducted by the subcommittee suggest areas and topics that ought to be reported:

1. Coverage error should be explicitly mentioned as a source of nonsampling error. McMillen and Brady (1999) provide a statement that is useful for short- format publications.
2. The target population should be defined and a clear statement made of exclusions to the population. The National Survey of Small Business Finance (section ####) provides a good illustration. Similarly, the National Household Education Survey (section ####) is quite specific about excluded populations. Descriptions of the characteristics and extent of the excluded populations should be provided.
3. The sampling frame should be identified and described. For example, see section ### for how this is treated for a random digit dial telephone survey.
4. An overall coverage rate should be defined and provided to the user. The Current Population Survey (Section ####) provides a good example.
5. Subpopulation coverage rates should be made available. The Current Population Survey (Section ####) illustrates how undercoverage in this household survey varies with age, sex, and race.
6. Postratification procedures should be described and the effects of using such procedures described. The National Household Education Survey case study (Section ####) provides an example of how this was handled for the National Household Education Survey.

2.6 Examples of Studies to Coverage Error

2.6.1 The Current Population Survey

The March 1996 Current Population Survey (CPS) is conducted by the U.S. Bureau of the Census. The March CPS uses the same target population and sampling frame as the monthly CPS. The target population consists of the civilian noninstitutional population ages 16 and older. Persons younger than 16 are not included in the definition of the labor force because children's labor participation is severely limited by law and compulsory school attendance. The institutional population, that is, those living in penal and mental institutions, medical facilities, homes for the aged, infirm, and needy are also excluded.

The CPS sampling frame is a master sample, which means that it is geographically based (as opposed to being a list of, say, telephone numbers). It is a composite frame derived from a number of already existing frames and geographic materials. In broad strokes, these frames are: (1) census blocks based on the most recent decennial census. Census blocks are grouped into three strata: unit, group quarters, and area, which were in existence at the time the most recent census was taken. (2) To capture housing units built after the census, a sample of building permits of new construction is included.

The March survey uses two sets of questions, the basic CPS and the supplement. The monthly CPS collects primarily labor force data about the civilian noninstitutional population. Interviewers ask questions concerning labor force participation about each member ages 16 and older for every sample household. In addition to the basic CPS questions, the supplementary questions asked in March concern money income received in the previous calendar year, educational attainment, household and family characteristics, marital status, and geographical mobility.

CPS undercoverage results from missed housing units and missed persons within sample households. Overall CPS undercoverage is estimated to be about 8 percent. CPS undercoverage varies with age, sex, and race. Generally, undercoverage is larger for males than for females and larger for Blacks and other races combined than for Whites. As described previously, ratio estimation to independent age-sex-race-Hispanic population controls partially corrects for the bias due to undercoverage. However, biases exist in the estimates to the extent that missed persons in missed households or missed persons in interviewed households have different characteristics from those of interviewed persons in the same age-sex-race-origin-state group.

A common measure of survey coverage is the coverage ratio, the estimated population before post-stratification divided by the independent population control. Table A shows CPS coverage ratios for age-sex-race groups for a typical month. The CPS coverage ratios can exhibit some variability from month to month. Other Census Bureau household surveys experience similar coverage.

Table A. CPS Coverage Ratios

	Non-Black		Black		All Persons		
Age	M	F	M	F	M	F	Total
0-14	0.929	0.964	0.850	0.838	0.916	0.943	0.929
15	0.933	0.895	0.763	0.824	0.905	0.883	0.895
16-19	0.881	0.891	0.711	0.802	0.855	0.877	0.866
20-29	0.847	0.897	0.660	0.811	0.823	0.884	0.854
30-39	0.904	0.931	0.680	0.845	0.877	0.920	0.899
40-49	0.928	0.966	0.816	0.911	0.917	0.959	0.938
50-59	0.953	0.974	0.896	0.927	0.948	0.969	0.959
60-64	0.961	0.941	0.954	0.953	0.960	0.942	0.950
65-69	0.919	0.972	0.982	0.984	0.924	0.973	0.951
70+	0.993	1.004	0.996	0.979	0.993	1.002	0.998
15+	0.914	0.945	0.767	0.874	0.898	0.927	0.918
0+	0.918	0.949	0.793	0.864	0.902	0.931	0.921

Notice that since the control totals are based on the census and administrative records, one can safely ignore their variance. The CPS itself has a measurable variance, but since these ratios are based on a recurring pattern of many months and years, it is reasonable to assume that the coverage ratios are due to bias rather than variance.

2.5.2 National Survey of Small Business Finance

The following example is abstracted from Cox et al. (1989).

> For the NSSBF, the target population was defined to be all nonfinancial and nonfarm small business enterprises in the U.S. in operation as of December 1987. A firm was considered to be small if it had fewer than 500 full-time equivalent employees.
>
> Nonfinancial and nonfarm business was defined as all privately owned and for-profit businesses, excluding industry groups: (1) agriculture, forestry, and fishing; (2) finance and insurance underwriting; and (3) real estate investment trusts.
>
> The sampling frame for the NSSBF was constructed from the December 1987 Dun's Market Identifier (DMI) file. The DMI frame has several desirable features as a sampling frame. Information on the DMI file includes the business address, telephone number, main office/branch status, standard industrial classification (SIC) code, and the name of the owner or principal executive officer. This information facilitates selection of the sample and establishment of contact with the appropriate person in the firm. Moreover, the DMI records file is updated regularly, making its obsolescence rate compare favorably with other publicly available sampling frames.
>
> To construct the sampling frame, all DMI records with ineligible SIC codes as well as records for branch offices and subsidiary companies were eliminated. The DMI variable for employment was not used to eliminate large businesses because it was often missing and was not defined in terms of full-time equivalents. Instead, information collected in the screening interview was used to eliminate firms with more than 500 employees and not-for-profit or publicly owned firms.
>
> To evaluate coverage issues further, we compared frame counts for establishments on the DMI file with statistics on business tax returns compiled by the Statistics of Income Division of the IRS (Internal Revenue Service 1988a, 1988b, 1987). Exact comparisons are not possible, but IRS statistics provide some basis for evaluating the DMI file's coverage of the universe of interest for this study.
>
> The DMI estimate for number of corporations is not much smaller than the IRS estimate. Both data sources indicate that the majority of corporations were engaged in retail trade and services. The distribution of corporations by industry groups is also similar, although the DMI list contains proportionately somewhat fewer service and real estate firms and

more manufacturing and trade firms that the IRS list. Hence, our preliminary work suggests that the DMI file's coverage of corporations is good.

The DMI estimate of the number of proprietorships, on the other hand, is substantially lower than the IRS estimate. In both lists proprietorships are more concentrated in the service and construction industries and less concentrated in manufacturing than either partnerships or corporations. The DMI list under represents proprietorships in all industry groups. The DMI file contains a greater proportion of trade firms and a smaller proportion of services than the IRS list.

A large part of the coverage problem for proprietorships appears to arise from a lack of coverage of businesses without employees. Businesses without employees are mostly proprietorships. They are often part-time businesses and are not easily identified. These firms include, for example, individuals for whom part-time self-employment is a secondary occupation. Zero-employee firms are a large proportion of the total number of businesses in the US, although they probably account for a small share of total revenues, assets, or employment. Other than tax return data, the most complete coverage of zero-employee firms is from household surveys such as the Current Population Survey or the Survey of Consumer Finances, which provide data on self-employment.

The DMI file's coverage of partnerships is not as good as its coverage of corporations but better than that of proprietorships. The most severe undercoverage of partnerships is found in the real estate industry. Again, the undercoverage of partnerships may be associated with zero-employee firms.

2.5.3 1995 National Household Education Survey (NHES:93)

The following material is abstracted from U.S. Department of Education, 1996. It demonstrates a best practice with respect to post stratification and reporting of errors.

The NHES is a telephone survey of the noninstitutionalized civilian population of the U.S. Households are selected for the survey using random digit dialing (RDD) methods, and data are collected using computer-assisted telephone interviewing (CATI) procedures. Approximately 45,000 to 60,000 households are screened for each administration, and individuals within households who meet predetermined criteria are sampled for more detailed or extended interviews. The data are weighted to permit estimates of the entire population. The NHES survey for a given year typically consists of a screener, which collects household composition and demographic data, and extended interviews on two substantive components addressing education-related topics. In order to assess data item reliability and inform future NHES surveys, each administration also includes a subsample of respondents for a reinterview.

The estimates from the National Household Education Survey of 1995 (NHES:95) are subject to bias because only households with telephones were sampled. Data from the 1992 October supplement to the CPS are used in this report to evaluate the potential size of the bias of the estimates. Since weighting adjustments are used in the NHES:95 with the goal of reducing this coverage bias, the findings in this report also provide an evaluation of the effectiveness of these adjustments.

The focus of this report is on the statistics for two separate populations: 0-to 2-year-olds who were sampled as a part of the Early Childhood Program Participation (ECPP) component and civilian adults who were sampled for the Adult Education (AE) component. Children from birth through 10 years old were sampled for the ECPP component, but previous research was already conducted for children aged 3 to 7 years using the same CPS data (Brick and Tubbs 1996). Thus, only the bias for statistics for children up to 2 years is included in this report.

Due to the potential biases due to undercoverage, the standard practice in the NHES is to make statistical adjustments of survey weights to compensate, to the extent possible, for undercoverage. The NHES adjustments that are specifically developed to compensate for the undercoverage are raking or post-stratification to known control totals that contain counts of persons living in both telephone and nontelephone households. The goal of these adjustments is to make the estimates from the survey consistent with known totals, to partially correct for undercoverage bias, and to reduce the variance of the estimates.

For this study, a slightly different procedure is used to produce adjusted weights that can be applied for the telephone households for the CPS to form estimates of all persons. Control totals of the number of persons in both telephone and nontelephone households were first produced from the CPS file separately for both 0- to 2-year-olds and adults eligible for the AE interview. The weights for the CPS respondents from telephone households were then raked to these control totals to produce adjusted weights that summed to the total number of persons in both telephone and nontelephone households. The responses from persons in telephone households are then used with these adjusted weights to produce adjusted estimates. The adjusted estimates can then be compared to the estimates from all persons in the CPS to assess the results coverage bias and this should be very similar to the coverage bias found in the NHES estimates.

The adjusted weights were applied to the observations from the respondents in telephone households to produce the adjusted estimates shown in the next to last column in tables 1...... The estimated bias in these statistics is given in the last column of these tables. The bias is the difference between the adjusted estimate and the estimate from all households. As before, a negative coverage bias indicates that the estimate is smaller than the estimate based on all households.

In general, the raking adjustments were effective in reducing the coverage bias of the estimates. The largest bias were generally smaller after the raking. Only a few of the estimates had bias estimates that were greater after the raking adjustment. However, the improvement was not uniform. For small estimates (2 percent or less) the raking adjustment had little benefit. This might have been expected, since the biases of these estimates before and after taking were all small. Little benefit could be expected from the adjustment in these circumstances. One subgroup for whom statistics were not improved by the raking adjustment was Hispanic children. The biases of the adjusted estimates for Hispanic children were as large or larger than the estimates before adjusting for all the estimates except for care by a relative.

Table 1

Estimated percentage of 0- and 2-year-olds by telephone status, estimated coverage bias, and adjusted coverage bias

Characteristics	Telephone Households	Non-telephone households	All households	Coverage bias	Adjusted telephone households	Adjusted coverage bias
Care Arrangements						
Care by a relative						
All	13.7	13.5	13.7	0	13.6	-0.1
Hispanic	13.4	10.8	12.9	0.5	12.6	-0.3
Black, non-Hispanic	20.5	17	19.6	0.9	19.4	-0.2
Nonblack, non-Hispanic	12.5	11.6	12.4	0.1	12.4	-0.1

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Situation On The 2000 American Population Census

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I. INTRODUCTION

The United States of America adopted a constitution in 1787 which required a census of the U.S. population every ten years starting in 1790. The first census was conducted in 1790. Methodologies to conduct a census, and to process and disseminate the data have changed since the first census. These methodologies continue to evolve with the changes in data needs, technologies and statistical theory. This paper presents Census 2000 methodologies with an emphasis on sampling and estimation methodologies.

II. BACKGROUND

From the time of the 1970 census, most people have been mailed or given a questionnaire, and asked to mail it back with the names of all the people in their household. To each household that does not return a questionnaire by mail, the Bureau sends an interviewer to collect the information. Although some households are visited many times, some people are not counted. There are many reasons that people are not counted in the census: no one may be at home, people move in or out, missed addresses, some people have no permanent address, the residence rules to identify who should be included on the questionnaire may not be clear to everyone, and some people do not want to be counted.

Following up 100 percent of the nonresponding households has been a major factor in the increase in census cost. This is due to the increase in the percentage of the households that require follow-up and in the number of personal visits. The response rate for the mail questionnaire declined from 78 percent in the 1970 Census, to 75 percent in 1980, to 65 percent in the 1990 Census (Memo. [1]). To address the issue of declining response rates and increased costs, the Bureau developed a plan to sample the nonresponding households.

Evaluation and research are integral parts of the decennial census. Evaluations are conducted to learn about the quality of census data and the research is done primarily to test new methodologies for the census. Evaluations following the 1990 census showed that the Bureau undercounted the total U.S. population, and that the net undercount varied by population subgroups. Robinson and Hogan (1990) stated that the undercount of persons in the U.S. decennial censuses has been recognized since the first census in 1790. Between 1790 and the 1960's, the issue of the census undercount as a national concern lay essentially dormant, with two exceptions being the concern over the alleged large undercounts in the 1870 and 1920 censuses.

Table 1 below shows net undercount rates by race based on the Post-Enumeration Survey. The Post-Enumeration Survey (PES) used capture-recapture methodology to estimate the total population. A detailed description of the PES methodology is given in Hogan (1993).

Table 1. Net Undercount Rates in the 1990 Census Based on the Post-Enumeration Survey

Race or Ethnicity	Owner	Renter	Total (standard errors)
United States Total	-	-	1.6% (0.2%)
White, non-Hispanic	-0.3%	3.1%	0.7% (0.2%)
Black	2.3%	6.5%	4.4% (0.5%)
Hispanic (any race)	1.8%	7.4%	5.0% (0.7%)
Asian/Pacific Islander	-1.4%	7.0%	2.3% (1.4%)
American Indian	-	-	4.5% (1.2%)

Source: 1990 Public Law Counts 94-171, and Hogan (1993).

Table 2 presents net undercount rates by black and non-black groups for the last six censuses as determined through demographic analysis. The demographic analysis (DA) methodology uses administrative data on annual births, deaths, migration, immigration and other state and local government records such as medicare enrollment, along with previous decennial census data. A detailed description of DA methodology can be found in Robinson et al. (1993).

Table 2. Historical Net Undercount Rates (in Percent) in the last six Censuses Based on Demographic Analysis

	1990	1980	1970	1960	1950	1940
Total	1.8	1.2	2.7	3.1	4.1	5.4
Black	5.7	4.5	6.5	6.6	7.5	8.4
Non-black	1.3	0.8	2.2	2.7	3.8	5.0

Source: Robinson et al. (1993).

Both tables show that the undercount varies by population subgroups. This phenomena is also called differential undercount by population subgroups. Also, the 1990 PES results (Public Law 94-171 adjusted data on the Bureau's website) show that children under 18 years of age have a higher net undercount rate (3.2%) than the persons 18 years or older (1.0%). DA shows that children 0-9 years of age have a higher net undercount rate than children 10-17 years of age (West and Robinson 1999). They also observed differences in coverage in census by other characteristics such as renter versus owner, male versus female etc.

Despite the Bureau's efforts, the differential net undercoverage has remained over the last six censuses. Also, evaluation studies showed that the mail return rate of questionnaires dropped in the 1990 census compared to 1980. We expect it to be lower still in Census 2000 and the cost for nonresponse followup per housing unit is increasing (Singh et al. 1999). Thus, to reduce the cost of the census and the differential net undercount by race and ethnicity, during the 1990's the Bureau developed an innovative plan for the Census 2000 which employed statistical sampling and estimation methodologies.

In 1998, the Census Bureau conducted a Dress Rehearsal, a test before the Census 2000, in three small areas of the country. In two of the sites, we implemented our plans for using statistical sampling and estimation procedures in the Census 2000. This included a post-enumeration survey called the Integrated Coverage Measurement (ICM). However, the use of these plans in 2000 was challenged in the courts. In Glavin v. Clinton, the U.S. Supreme Court ruled on January 25, 1999 against the use of sampling and estimation for the apportionment of Congressional seats. On the other hand, it allowed the use of sampling and estimation for all other purposes. Based on this ruling, the Census Bureau will conduct an Accuracy and Coverage Evaluation (A.C.E.) survey, analogous to the PES and ICM, as part of the Census 2000. This paper discusses the Census 2000 plans before and after the Supreme Court ruling. Both plans incorporate many improvements made since the 1990 Census to ensure a successful Census 2000. The differences in the two plans are summarized in Section IV.

III. CENSUS 2000 PLANS PRIOR TO SUPREME COURT RULING

Prior to the Supreme Court ruling, the Census Bureau's plan for the Census 2000 had two primary phases--the initial phase and the Integrated Coverage Measurement. These are discussed below.

III.A. The Initial Phase

The initial phase included

Operations involving address creation and data collection:

- Creation of address list.
- Local Update of Census Addresses (LUCA) program to improve coverage by creating an accurate census address list.
- Strong partnership program with local communities to improve Bureau's address list and encourage participation in the census.

Procedures to ensure complete collection of census data:

- Multiple mail contacts with occupants of residential address units.
- A toll free number for assistance, requesting a census questionnaire and call in to complete a questionnaire on phone.
- Blank forms (Be Counted Forms) available at many convenient locations to be completed by those who did not receive a form or believed that they were not counted in census.
- Census questionnaire on Internet.
- Strong advertising and community-based publicity program to encourage participation of community members.
- A program to identify duplicates due to multiple opportunities to respond to the census.

Statistical sampling and estimation procedures:

- The Long Form Sample, a scientific statistical sample with an overall rate of about 17% to collect detailed socio-economic data. (This will not be discussed in the paper.)

- Sampling for nonresponse follow-up
- Sampling for undelivered-as-addressed vacant housing units
- Estimation for service based enumeration

The last three items above are briefly discussed below:

III.A.1. Sampling for Nonresponse Follow-up (NRFU)

The Bureau originally planned to select a systematic random sample of nonresponding housing units separately in each census tract. The nonresponding housing units are those for which a questionnaire was delivered but the Bureau did not receive a questionnaire back from them. The goal for NRFU sampling was to achieve a completion rate of 90% or higher in each tract. Thus the sample rate depended on the initial response rate, and varied by tract; as the response rate increased, our sampling rate in that tract generally decreased. The sampling rate in a tract was determined as

$$\text{Sampling Rate} = \frac{0.90 - \text{initial response rate}}{1.00 - \text{initial response rate}},$$

where the initial response rate was the number of self-responding addresses and postal returns divided by the number of addresses mailed or delivered a questionnaire. Thus, if the initial response rate in a tract was 70%, then the sample rate would be 0.20/0.30, or 2 in 3. In general, if the initial response rate was 85% or higher, we used a 1-in-3 sampling rate.

Before selecting the sample for NRFU, the nonresponding housing units were sorted within the tract by geography and form type (long vs. short form). Then a systematic sample was selected. This procedure ensured that the sample was distributed evenly across the tract.

The population characteristics of the remaining nonresponding households--those not selected in the sample--were imputed using a hot-deck procedure based on information collected from sampled nonrespondents in the same census tract. (This was also true for housing units selected in the NRFU sample who were nonrespondents again in the follow-up.) The procedure was designed to reduce bias in estimation and to ensure that the hot-deck population estimates agree in expectation with simple weighted estimates at the tract and higher levels of geography.

Occasionally a census form was returned after NRFU sample selection. The information from these forms was not discarded; rather an appropriate adjustment to the estimation methodology was made to accommodate late forms. For details, see Farber and Griffin (1998), and Griffin and Vacca (1998).

III.A.2. Sampling the Undeliverable-As-Addressed (UAA) Vacant Returns

For many of the housing units, we expected the U.S. Postal Service to return their questionnaires as "undeliverable as addressed." The Bureau planned to selected a 3-in-10 sample of the UAA vacant housing units for personal visits to check whether the housing unit was actually vacant. This sampling rate was the same in each tract, regardless of the number of returns. Before selecting the sample, the vacant returns were sorted by geography and form type (short vs. long) within an eligible tract. The characteristics for UAA vacant units not selected into the sample were imputed based on sample cases in the same census tract. For details, see Farber and Griffin (1998), and Griffin and Vacca (1998).

III.A.3. Service Based Enumeration (SBE) and Multiplicity Estimation

An operation called SBE is designed to provide people with no usual residence an opportunity to be enumerated. The Bureau planned special procedures to enumerate such persons at shelters, soup kitchens, mobile food vans, and certain outdoor locations with no apparent means of shelter. Because the SBE can only account for people at these facilities on the day of enumeration, the Bureau planned to apply multiplicity estimation to account for people who sometimes use these facilities but did not use them on the day of the SBE enumeration. A brief description of multiplicity estimation is given below.

During the interview of persons at shelters, soup kitchens, mobile food vans, and other specific outdoor locations, the multiplicity question was asked: *How many days during the past week, including today, have you visited shelters, soup kitchens, or mobile food vans?* The multiplicity estimator for the number of people using these facilities is given by

$$\hat{N} = \sum_{k=1}^{M} \frac{7}{A_k}$$

where, A_k is the number of times the kth person used the facility during the week, and M is the number of people enumerated. Assigning each enumerated person a count of $7/A_k$ allows us to estimate how many people are using these facilities but were not enumerated that day.

Meanwhile, the information from the SBE questionnaire also allows us to remove from the estimator (when appropriate) people who use multiple facilities during the enumeration week. Of those persons enumerated on questionnaires from soup kitchen and mobile food vans, we counted only those who did not use shelters and those who did not respond to the multiplicity question. Removing people with multiple chances of enumeration allows us to maintain an expected enumeration value of about 1. For more information on multiplicity estimation, see Kohn and Griffin (1999) or Shores, Cantwell, and Kohn (1999). Once the estimator $\hat{N}$ is obtained for the group using these services, records for the estimated $\hat{N} - M$ persons are imputed from those who were enumerated.

III.B. Integrated Coverage Measurement (ICM)

The purpose of the Integrated Coverage Measurement in the Dress Rehearsal was to measure the total population of each state and also correct for the differential net undercount in population estimates, particularly by race and ethnicity. The Bureau used the ICM sample design to select initial A.C.E. block clusters; this is discussed in Section IV. Hence, to save space in this paper, we will present a brief summary of the ICM plans below, and will postpone the detailed discussion to the section on A.C.E. A more thorough development of the ICM methodology can be found in Waite and Hogan (1998).

III.B.1. ICM Sampling

For Census 2000, the Bureau originally developed a state-based design for the ICM with a sample size of about 750,000 housing units spread across the country to yield state total population estimates with a coefficient of variation of 0.5% or less.

The ICM sample was to be a stratified, systematic sample of geographical areas called block clusters. Once ICM clusters were selected, the Census Bureau field staff would list all housing units in the sample block clusters independently, that is, without the use of any census address lists. If a selected cluster had fewer than 80 housing units in the independent listing, all would be retained for ICM computer-assisted personal interviewing. If the cluster had 80 or more housing units on its independent list, the cluster was to be divided into segments and one or more of these segments would be selected randomly. This was planned to make the interviewer work load more efficient and to improve the efficiency of the design by reducing the clustering effect. Other adjustments (such as reducing the number of sample clusters) to the sample were also applied to bring field workloads in line with expectation. For a detail discussions, see Section IV.E.

III.B.2. ICM Estimation

An important feature of ICM design was to produce a direct state estimate. In all discussions in this section, we assume that data from one state would not be used for the estimates in any other state. Estimation in the ICM entailed several major steps. A brief description of each is given in the following subsections.

Our plan was to apply a statistical method based on the theory referred to as "capture-recapture." The results from the ICM interviews would be matched to the initial-phase census results to determine how many people were missed or were erroneously enumerated in the initial phase. Data from the ICM and the initial phase would then be integrated to estimate the total population in each state and the U.S.

We planned to use dual system estimation (DSE) to correct coverage error in the census. Because capture probabilities are not the same for all members of the population, we planned to partition the population into groups called estimation domains such that coverage probabilities are similar for all members in a domain but different in different domains. The dual system estimates are then calculated separately in each domain. These post-strata were to be defined based on combinations of tenure, race, ethnicity, age, and sex separately within each state. The detailed DSE methodology is presented later in this paper.

For a given poststratum, the coverage factor is defined as the ratio of the dual system estimate divided by the census count. This coverage factor allows us to compute small-area estimates at the block level using an approach called synthetic estimation. For people satisfying the characteristics of the poststratum, the block-level estimate (for these people) is obtained by multiplying the corresponding census count by the poststratum coverage factor. A controlled rounding procedure is then applied to obtain integer person estimates. For information on these and other more intricate aspects of the dual system estimator as applied to the 1990 Post-Enumeration Survey, see Hogan (1992; 1993).

IV. CENSUS 2000 PLANS AFTER SUPREME COURT RULING

The Supreme Court ruling does not allow statistical sampling and estimation for apportionment counts, but does for other purposes. To abide by the law, the Bureau modified its census plans. Under the current Census 2000 plans, the Bureau will enumerate the U.S. population using a traditional type of census to produce U.S. population counts for the apportionment of the congressional seats and will conduct the Accuracy and Coverage Evaluation (A.C.E.) to produce estimates for all other purposes.

The following discussion summarizes plans noted in the "Census 2000 Operation Plans" dated January, 1999. The traditional census plan is designed to compensate to the extent possible for the gains that can be achieved through the application of modern statistical sampling techniques. It is presented as an alternative to a census design employing sampling methods, but this alternative cannot achieve the overall accuracy, reduction in the differential undercount of minorities and children, or cost-effectiveness of census that also includes statistical sampling. Regardless of which methods are ultimately implemented, the Census Bureau expects to conduct Census 2000 in a throughly professional manner to meet the Nation's needs for demographic data.

While both plans--before and after the Supreme Court Decision--incorporate the many improvements made since the 1990 census to ensure a successful Census 2000, this summary highlights how the traditional census plan differs from the census plan with sampling. The differences can be summarized into five major categories:

- Methods to improve public response
- Methods for conducting the nonresponse follow-up and UAA operations without sampling
- Methods to improve coverage in lieu of the use of sampling techniques
- Quality assurance and training enhancements to already planned methods
- Accuracy and Coverage Evaluation survey to evaluate the census and produce numbers for non-apportionment purposes

These five major categories are briefly described below.

IV.A. Methods to improve public response

We focus on two principal components: expansion of the partnership program, and expansion and enhancement of the paid advertising and promotion program. Both programs are geared toward greater public awareness of the census, which in turn should lead to greater public response and cooperation. Greater public participation improves data quality and reduces personal visits for follow-up. This is particularly important for a traditional approach to the census, which does not use statistical sampling to reduce the work load for collecting data by personal visits to households that do not respond by mail.

- An expanded partnership program would allow the Bureau to form additional partnerships with both non-governmental organizations that represent historically hard-to-enumerate groups and with governmental entities, including tribal governments, that have not yet taken the opportunity to be included in the partnership program. The expanded program also includes "in-kind funding" to support partners by providing services, such as printing locally designed Census 2000 promotional materials.

- The expanded and enhanced paid advertising and promotion program includes developing and implementing additional advertising messages. One additional message, which will be deployed prior to Census Day, targets information about community benefits to areas with historically low participation in the census. Another message seeks the public's cooperation with enumerators during the nonresponse follow-up operation. The promotion program also would expand "Census in the Schools," allowing all schools to participate instead of only those in selected areas. Nontraditional advertising methods also would be pursued. Fact sheets and promotional materials will be available on a larger scale with the expanded promotion program. And finally, special publicity events are planned that would bring the census message to communities across the Nation.

IV.B. Methods for conducting the nonresponse follow-up operation without sampling

One hundred percent of the nonresponding households will be followed up by census enumerators. Due to the substantial increase in work load resulting from the inclusion of 100 percent rather than a sample of nonrespondents, the following changes will be implemented:

- The time frame for conducting the nonresponse follow-up operation will be expended to allow more time for enumerators to contact households and complete questionnaires.
- Additional census offices will be opened in order to implement, control, and support this expanded operation.

IV.C. Methods to improve coverage

There are many enhancements included in the traditional census plan to try to improve coverage and, to the extent possible, compensate for some portion of the benefits that sampling techniques could provide. A brief discussion is presented below:

- Addresses classified as vacant, nonexistent, and nonresidential by census enumerators during the nonresponse follow-up operation would be followed up in a separate field operation to validate their classification, unless previously confirmed by the U.S. Postal Service or another enumerator. This coverage improvement method would allow residents at these addresses to be enumerated if a mistake in classification had been made.
- The plan also includes a review of all questionnaires to determine whether persons are appropriately counted at each address. A follow-up phone call will take place for those questionnaires which indicated that persons may have been omitted. This review is much more extensive than that planned for the design which uses sampling.
- We would expand our "tool kit" of methods used for targeting areas that need additional help to gain cooperation or coverage. We would make available to the Regional Census Centers a planning database, which will enable them to predict areas that will benefit from such methods. In consultation with local partners, we would then identify where each tool will be employed in advance of Census Day.
- Finally, the plan does not include either the use of administrative records of individual persons or housing units to improve the coverage of the census, or an opportunity for local and tribal officials to review housing unit counts following the enumeration. We did not include these programs, because at this point in our analysis they do not appear to offer an effective way to improve census coverage.Instead, we will conduct LUCA which is more productive than the local review following the enumeration.

IV.D. Quality assurance and training enhancements to already planned methods

- For some existing operations, such as update/leave, there would be enhanced training for enumerators to improve quality of operations. For some operations, the training would be augmented so enumerators could gain more experience in certain aspects of the job that have caused difficulties in the past.

- Several of the existing field operations will have quality assurance programs added or enhanced. Nonresponse follow-up, update/leave, and group quarters enumeration are included in this category.

IV.E. Accuracy and Coverage Evaluation (A.C.E.)

The A.C.E. is based on the design of the ICM.. The primary differences are the sample size and the schedule for producing the A.C.E. estimates. Prior to the January 1999 Supreme Court ruling, the ICM was planned as a sample of 750,000 housing units. A key feature for the ICM design was the ability to produce direct state estimates with acceptable reliability. This feature had a direct impact on how the 750,000 units were to be allocated to each state and how the estimation poststrata were to be defined. The allocation of the ICM sample (Schindler 1998) was designed to produce acceptable relative accuracy for each state estimate and for the apportionment result. The ICM estimation poststrata were not permitted to cross state boundaries. Thus, the challenge was to develop poststrata for each state that could be supported by the state's sample. Consequently, most states--except for the most populous--were allocated roughly equal sample sizes.

The primary sampling unit was a block cluster, a group of contiguous blocks with about 30 housing units. For the most part, the ICM sample was to be proportionally allocated within each state. The ICM design included a separate allocation of 355 sample block clusters on American Indian Reservations and Trust Lands. By the time of the Supreme Court decision, we had already committed to some of the features of the ICM sample design. This imposes some constraints on the redesign effort, particularly for the allocation of the A.C.E. sample.

The Census Bureau currently plans to develop an A.C.E. with a reduced sample size of approximately 300,000 housing units. The goals are to allocate the 300,000 units to achieve reliability for race/Hispanic origin/tenure (tenure: own vs. rent) groups across states while still attempting to maintain each state's reliability. Our plans include grouping the persons together with the similar coverage properties during the estimation process without regard to their place of residence. State estimates will be produced by a form of synthetic estimation more like the 1990 PES than the direct state estimates that had been planned for the ICM. The 1990 PES synthetic estimation took into account Census region (Northeast, Midwest, South, and West), race, ethnicity, a measure of urbanicity, tenure, age and sex. This will allow the estimation process to use information obtained from persons in different states with similar coverage properties and thus improve the effectiveness of the state estimates, particularly for some demographic subgroups.

We discuss below two key components--sampling and estimation--of the A.C.E. design for Census 2000.

IV.E.1. A.C.E. Sample Plans

Reducing the originally planned sample of 750,000 housing units for the ICM to 300,000 for the 2000 A.C.E. is an operational necessity that imposes some constraints. Overall, we expect the reliability to be better than the 1990 PES for the majority of poststratum estimates. There will, however, be several estimation domains that will be comparable or perhaps slightly less reliable than in the 1990 PES, but overall we expect the state estimates to be more reliable than in the 1990 PES.

The sampling for the A.C.E. will occur in phases:

- An initial sample of block clusters allocated according to the 750,000 ICM design.
- A reduction in number of block clusters to reduce sample for A.C.E. design
- A reduction of the sample within large block clusters to help achieve target sample for the A.C.E.

IV.E.1.a. Initial Sample of Block Clusters

The initial sample selection has three main steps which are discussed below.

Forming Block Clusters

Forming block clusters is the first step in selecting the sample. We form block clusters by combining adjacent blocks with at least three housing units such that a cluster has about 30 housing units but no more than 79 housing units. A block with fewer than three or more than 79 units was defined as a block cluster by itself. In certain cases clusters with fewer than three housing units are combined with other clusters.

Sampling Strata

There are four sampling strata for the initial sampling. Three of these strata are based on the size of a cluster in terms of the number of housing units: small (0-2), medium (3-79), and large (80 or more). The fourth stratum includes medium and large clusters on American Indian Reservations (AIR) and Trust Lands. Remote Alaskan and the population living in group quarters (GQs) will not be part of the A.C.E. universe and, hence, will not be sampled.

Selection of Sample

Sampling is done independently in each state, the District of Columbia and Puerto Rico. The sample, in general, was allocated to each state based on how the estimation domains were to be defined. Most states were allocated roughly equal sample sizes except for the most populous ones. In general, the ICM sample was to be proportionally allocated within state.

To ensure a reliable estimate for American Indians (AIs), the sample design included a separate sample allocation of 355 block clusters to the American Indian Reservation and Trust Lands (AIR). Before selecting a sample, a cluster is assigned to one of the twelve race/tenure groups based on a prespecified criterion. The six groups for race are Hawaiian and Pacific Islander, American Indian and Alaska Native, Asian, Hispanic, Black, and White and others. The tenure groups are renter and owner.

Within each state a systematic random sample is selected within each sampling stratum. The clusters are sorted in a stratum within the following order:

- American Indian Country indicator
- race/tenure group assignment
- 1990 Estimated Urbanization
- County
- Geographic Block Cluster Number

Differential sampling is used across the four sampling strata. Small block clusters are generally sampled at a lower rate than medium and larger clusters. Large blocks are sampled at a higher rate so that subsampling within these blocks will produce an efficient design. The initial sample of block clusters has been completed. A summary of the results of the initial sampling phase is presented in table 3. below.

Table 3. Summary Table of Initial Sample Results

Sampling Stratum	Universe		Listing Sample	
	Block Clusters	Housing Units[1]	Block Clusters	Housing Units[1]
Small	1,023,384	330,200	5,000	2,400
Medium	2,451,081	71,577,000	15,393	438,600
Large	247,810	45,091,000	8,388	1,539,800
American Indian Reservation	12,618	370,800	355	8,620
Total[2]	3,734,893	117,369,000	29,136	1,989,420

[1]Housing unit counts are preliminary and will differ from actual housing unit counts.
[2]Totals do not include Puerto Rico.
Source: Kostanich (June 25, 1999)

The independent listing of housing units in sample blocks is under preparation. The methodology for the remaining components is currently being researched and will be finalized soon.

As shown in table 3 above, the sample originally planned for the ICM will contain approximately two million housing units (HUs). The Bureau will reduce this sample to an A.C.E. sample of about 300,000 HUs. This will be accomplished by reducing i) the number of small, medium, and large block clusters, and ii) subsampling housing units in large blocks such that it provides a sufficiently large sample for each estimation domain to be used in DSE. A brief discussion of sample reduction methodologies is presented below.

IV.E.1.b. Reduction in Number of Block Clusters

The first step in reducing the sample is to reduce the number of block clusters selected in the initial phase. Below is an overview of the methodology to sample clusters.

AIR Block Cluster

In order to provide a reliable estimate for the AIR population, all 355 AIR block clusters will be retained. The 355 clusters were allocated to 26 states based on the American Indian population living on AIR and Trust Lands. Ten states did not have any clusters allocated due to their very small AIR retained. The 355 clusters were allocated to 26 states based on the American Indian population living on AIR and Trust Lands. Ten states did not have any clusters allocated due to their very small AIR population. However, one small cluster in AIR was selected as a part of the regular sampling in one of these ten states. This cluster will not be eligible for subsampling.

Small Block Clusters

Small clusters (technically, clusters with small measure of size) are very expensive to enumerate and have small populations, yet they have a potential to contribute significantly to the variance of coverage error estimates. Thus, the Bureau will reduce the number of these clusters in the A.C.E. from the originally selected 5,000 (approximately) in such a way that it has a smaller affect on variance. To accomplish this goal, the number of housing units on the independent listing will be compared to the number on the Decennial Master Address File (DMAF). The differences will be used to select a differential sample of small clusters for A.C.E.

For example, if the independent listing for a block has a larger number of housing units than the DMAF, we may keep such a block in the A.C.E. sample with certainty or with high probability. On the other hand, if the independent listing has about the same number of housing units as the DMAF, we will retain the cluster with a much lower probability. The Bureau continues to investigate its subsampling methodology to reduce the number of small block clusters in A.C.E. sample.

Medium and Large Block Clusters

For medium and large block clusters, only those that are not on an American Indian Reservation and not in Puerto Rico are subsampled in the A.C.E. reduction. The calculation of reduction sampling rates here is based on the most recent measure of size, the preliminary housing-unit count for the A.C.E. independent listing. These counts are preliminary because the number is simply a clerical tally of the number of housing units recorded in the independent listing book.

A.C.E. block-cluster reduction is still being studied. Currently we are researching reduction via differential rates rather than proportional sampling to provide more reliable estimates for population subgroups. To reduce the variances, differential sampling is being investigated for demographic groups that have been traditionally undercounted, and for clusters where the housing unit counts differ significantly from the census to the A.C.E. independent listing. Research will also help determine what the differential subsampling rates should be to maximize variance reduction while controlling weight variation.

IV.E.1.c. Reduction of Sampling Units Within Large Block Clusters

The ICM sample will be reduced further by subsampling within each remaining large cluster. Besides providing a desired A.C.E. sample size, it will increase the efficiency of the design by reducing the clustering effect of large blocks. In a cluster with more than 79 housing units, the cluster will be divided into two or more approximately equal segments of adjacent units. Then a sample of segments will be selected by taking a systematic sample across all large block clusters in a state. This process will also help us achieve a target sample for the A.C.E.

Block clusters that were in the medium stratum for the initial sample selection, but are seen to have 80 or more housing units based on the preliminary listing, probably will not be subsampled. Subsampling of housing units in these clusters will increase the variance by increasing the amount of block-cluster weight variation.

IV.E.2. P and E Samples

The people living in the housing units in A.C.E. sample block clusters make up the P Sample. In these clusters the Bureau will also identify the housing units and person records enumerated in the census, that is, the E Sample. The purpose here is to measure the extent of matches in the P sample, and correct (and erroneous) enumerations in the census. We determine the E Sample in the same blocks as the A.C.E. for reasons of cost and efficiency. In small and medium blocks and AIR blocks of the A.C.E. sample, persons enumerated in the Census will constitute the E Sample for these three strata. For large cluster blocks, we will map the A.C.E. segments in these clusters onto the census address list. If this yields more than 80 housing units in the E Sample in any cluster, the units may be subsampled in that cluster.

IV.E.3. Treatment of Movers

Some people will move between Census Day and the A.C.E. interview day. In the A.C.E. interview we will use a procedure that identifies all current residents living or staying at the sample address at the time of the A.C.E. interview as well as all other persons who lived at the address on Census Day and have moved away since Census Day. For out-movers, the interviewers will attempt a proxy interviewer to obtain data such as name, sex, and age that can be used for matching. We estimate the match rate of movers by using out-movers, while we estimate the total number of movers using data from in-movers.

IV.E.4. Missing Data

To estimate the net undercount, it is critical to measure (i) the rate of erroneous enumerations in the initial phase of the census, and (ii) the rate of P-sample matches to census enumerations in the A.C.E. block clusters. Follow-up operations will be used to determine erroneous enumerations by identifying duplicates, geocoding errors, fictitious persons, and illegible names. There operations will also be used to determine if a non-matched person was correctly enumerated.

For some people in either sample, the information collected will be insufficient to resolve the appropriate status. In such cases, the probability of a match or correct enumeration will be assigned through ratio estimation based on the corresponding rates from similar people whose status was resolved. A similar procedure will be used to assign a residence status (resident or nonresident on census day) to people in the P sample with insufficient information. This is necessary because of the procedure applied to handle cases where people moved in or out of the housing unit after Census Day. Various aspects of the methodology to handle missing data in A.C.E. are currently being researched. Methodology used in Census 2000 Dress Rehearsal is given in Hefter et al. (1999), and Kearney and Ikeda (1999).

In addition, a ratio adjustment at the cluster level was applied during the Census 2000 Dress Rehearsal to account for whole households that were nonrespondents in the ICM. We used hot-deck methodology to estimate missing characteristics (such as race, sex, and age) needed to form estimation domains. We plan to use the same methodology for Census 2000.

IV.E.5. Estimation Domain

We will use dual system estimation for correcting the coverage error in the census. The technique is based on capture-recapture methodology. Because capture probabilities are not equal for all members of the population, we try to partition the population into groups, called poststrata or estimation domains, such that the coverage probabilities are similar for all members in the group but different for members of different groups. These domains will be required to have a minimum population size; if not, groups will be collapsed according to predefined criteria.
Research is still underway to determine the estimation domains to be used in the A.C.E. Among the variables currently under consideration are

- Census Region: Northeast, South, Midwest, and West parts of the nation
- Census Division: further breakdowns of the Census Regions
- Race/Hispanic origin:(1) Non-Hispanic White & Other, (2) Black, (3) Non-Black Hispanic, (4) Asian & Pacific Islander, and (5) American Indians on Reservations; or a similar partitioning.
- Age/Sex: (1) under 18, (2) 18-29 male, (3) 18-29 female, (4) 30-49 male, (5) 30-39 female, (6) 50+ male, and (7) 50+ female.
- Tenure: (1) owner and (2) renter
- Type of Enumeration Area (TEA): (1) Tape Address Register (TAR) and (2) Prelist Pocket, Update Leave, and List/Enumerate
- Urbanicity: (1) urbanized areas > 250,000 persons, (2) other urban and (3) non-urban areas.
  Note: Urban/rural definitions will not be available in time for production poststratification for Census 2000 A.C.E. Thus if urbanicity is determined to be an important variable, a revised definition will be needed.
- Percent owner (block-level variable): (1) low and (2) other. Low percent owner blocks are those in the bottom 25th percentile based on percent owners.
- Mail-response rate (block level): (1) low and (2) other; uses the proportion of households in the 1990 mail universe that completed their census form without the aid of an enumerator. Low mail-response-rate blocks are those in the bottom 25th percentile based on mail-response rate.
- Percent minority (block level): (1) high and (2) other. High percent minority blocks are those in the top 75th percentile based on percent minorities.
- Household size: (1) one and (2) two or more.
- Household composition: (1)Type one and (2) Type two based on relationship to coverage.

IV.E.6. Direct Dual System Estimation and Correction Factor

We have defined data from the census enumeration and the A.C.E. sample in the sample clusters as E-sample and P-sample data, respectively. The Census Bureau will match P-sample persons to E-sample persons to classify each person as being included (or not) in the census enumeration, and as being included (or not) in the P-sample as follows:

Table 4. Enumeration in the Census and in the A.C.E.

P-Sample (A.C.E.)	Census Enumeration		Total
	In	Out	
In	N_{11}	N_{12}	N_{1+}
Out	N_{21}	N_{22}	N_{2+}
Total	N_{+1}	N_{+2}	N_{++}

Except for N_{22}, all internal cell values are observable. Hence, under the assumption of independence between inclusion in the census and in the P sample, we model the DSE population total as:

$$N_{++} = \frac{(N_{+1})(N_{1+})}{N_{11}} \qquad \text{(Wolter, 1986)}$$

In the DSE model, N_{+1} is the number of distinct and identifiable census persons. This is also called the official census count. The official census count, however, includes erroneously enumerated persons and imputed persons. Hence, we exclude whole-person census imputations from the E sample, and then evaluate the E sample to estimate and adjust for the proportion of erroneous census enumerations.

Thus, the DSE estimate for a given poststratum is as follows:

$$\hat{N}_{++} \triangleq \left\{ \frac{\left[(N_c - II)(1 - \hat{E}E / \hat{N}_e)\right]\hat{N}_p}{\hat{M}N_c} \right\} N_c = AF_d N_c$$

where

	$\hat{N}_{++} =$	dual system estimate of population
	$\hat{N}_p =$	weighted P-sample total, which estimates N_{1+}
	$N_c =$	total population enumerated in census
	$II =$	number of whole-person initial phase imputations
	$\hat{E}E =$	weighted estimate of E-sample erroneous enumerations
	$\hat{N}_e =$	weighted E-sample total
	$\hat{M} =$	weighted estimate of P-sample matches, which estimates N_{11}

$AF_d =$ estimated DSE adjustment factor

People in some A.C.E. households will move out from their Census-Day residence. We will not follow the whole household movers but will conduct proxy A.C.E. interviews at their Census-Day addresses. We will also identify and interview all households living or staying at the sample address at the time of the ICM interview who were not living there on census day. We will attempt to match only the Census-Day residents of sample addresses and will estimate the number of movers using in-movers after Census Day. We will use this approach, called PES-C, due to the lower tracing rate of out-movers.

Because we will use PES-C in the Census 2000, we estimate, $\hat{M}$ as $\hat{M}_{NM} + \frac{\hat{M}_{OM}}{\hat{N}_{OM}} \hat{N}_{IM}$

$$\hat{N}_P = \hat{N}_{NM} + \hat{N}_{IM}$$

where the subscripts $_{NM,\,OM,}$ and $_{IM}$ refer to non-mover, out-mover and in-mover, respectively.

IV.E.7. Small Area Estimation

Simple synthetic estimation will be used to estimate the population down to the block levels within each poststratum. The total population estimate for any geographic level is the sum of estimates over all poststrata within that level. A controlled rounding will be used to obtain integral number of people and consistent estimates at different geographic levels.

V. SUMMARY

After the Supreme Court decision in January, 1999, the U.S. Census Bureau revised the Census 2000 plans to produce the best Census ever. The revised traditional census plan is designed to compensate to the extent possible for the gains that can be achieved through the application of modern statistical sampling techniques. It is presented as an alternative to a census design employing sampling methods, but this alternative cannot achieve the overall accuracy, reduction in the differential undercount of minorities and children, or cost-effectiveness of census that also includes statistical sampling. The Census 2000 plan includes a number of programs to encourage participation in census, and improve coverage and the quality of the census. It also includes an A.C.E. to measure coverage error and to adjust the counts for all purposes other than apportionment of the Congress. The Census methodology is statistically sound and has undergone critical review by external experts including the National Academy of Science. For all purposes other than apportionment of Congressional seats, the data will be released at all geographic levels with and without corrections for coverage error.

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The 1990 Post-Enumeration Survey: Operations and Results

HOWARD HOGAN*

The Census Bureau has struggled for decades with the problem of undercount in the population census. Although the net national undercount has been greatly reduced in recent censuses, it still tends to display important differences by race, ethnic origin, and geographic location. The 1990 Post-Enumeration Survey (PES) was designed to produce Census tabulation of states and local areas corrected for the undercount or overcount of population The PES was the subject of litigation between the federal government and a coalition of states and local governments. Because of the litigation, the PES was conducted under specific guidelines concerning timing, prespecification, and quality. The PES measured Census omissions by independently interviewing a stratified sample of the population. It measured Census erroneous enumerations by a dependent reinterview of a sample of Census records and by searching the records for duplicates. A dual-system estimator (DSE) was used to prepare estimates of the population by post-strata. Adjustment factors were computed as the ratio of these estimates to the census count These factors were smoothed using a generalized linear model and then applied to the census counts by block and post-strata to produce adjusted census estimates. Although the government decided not to release these numbers as the official census results, the Census Bureau has conducted further research to improve these estimates to incorporate them into the postcensal estimates program. The revisions have included new post-strata and corrections of errors found in the original estimates. The results of the PES show a differential undercount by race and ethnic group and by owner/nonowner status They also demonstrate differences in undercount by geography.

KEY WORDS. Census; Dual-system estimates; Linear models, Undercount

1. INTRODUCTION

The U.S. Census Bureau has a long tradition of using statistical and demographic methods to evaluate the coverage of the population census (see for example, U.S. Bureau of the Census 1988). But for more than a decade there has existed a political and legal controversy about whether those same methods could or should be used to adjust the census results for the undercount. This controversy led to litigation, which in 1989 culminated in an agreement between the U.S. Department of Commerce (of which the Census Bureau is part) and a coalition of states, cities, and organizations led by New York City. According to that agreement, the Census Bureau was to conduct a post-enumeration survey (PES) and prepare for an adjustment. The Secretary of Commerce, however, reserved the final decision of whether to certify the original or the adjusted census results as official. Guidelines were published for making that decision (see U.S. Department of Commerce 1990).

The 1990 PES was the method chosen to produce census tabulations for states and local areas corrected for population undercount or overcount. Because of the guidelines, the PES had to meet several requirements. It needed to produce the estimates to be used to correct the census no later than May 1991, just over a year after Census Day, April 1, 1990. From these estimates, corrected census tabulations had to be produced by July 15, 1991. The PES also needed to meet high quality standards in terms of missing data, matching errors, and other nonsampling errors. Further, to lessen the possibility of (as well as the appearance of) political or other manipulation of the adjusted census results, procedures had to be specified before analyzing the data. Finally, the PES was to be judged not simply by its ability to estimate the national population, but also by its ability to produce improved estimates for states and local areas.

On July 15, 1991, the Secretary of Commerce announced his decision not to adjust the census. His reasoning was set forth in the *Federal Register* (U.S. Department of Commerce 1991). The decision has sparked renewed controversy among statisticians, as well as further litigation in the Federal courts. As part of his decision, the Secretary asked the Census Bureau to investigate using the PES results to correct the population estimates it makes each year following a census. These postcensal estimates are used as statistical controls for various demographic surveys and for distributing federal funds under several programs. Thus the story of the adjustment has two parts: the work conducted before July 15, 1991, designed for possible adjustment of the census and the subsequent work aimed at possible adjustment of the postcensal estimates. Further, the PES also serves the traditional purpose of census evaluation.

The next section discusses the process that produced the census adjustment estimates This section is followed by a discussion of the work aimed at improving the estimates conducted since the adjustment decision. The article then presents some of the principal results, including both the results available at the time of the adjustment decision and results produced since then.

2. PREPARING FOR CENSUS ADJUSTMENT

2.1 Overview of Design

The 1990 PES consisted of two parts. The first part was a sample of the population, known as the *P* sample. The proportion of the *P* sample included in the census is an estimate of the proportion of the total population included in the

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census. The second part consisted of a sample of the census enumerations used to estimate the proportion of erroneous census enumerations. This sample is known as the E sample. These enumerations were checked against the census itself to determine the extent of duplication. They were also checked in the field to determine the extent of fictitious enumerations, inclusions by the census of people born after the census reference day, and the extent to which people were counted in the wrong location.

The population was divided into post-strata based on geography, race, origin, housing tenure, age, and sex. The post-strata were based (roughly) on the following hierarchy:

Region (4)
 Northeast, South, Midwest, West
Census Division (9)
 New England, Middle Atlantic, South Atlantic, East South Central, West South Central, East North Central, West North Central, Mountain, Pacific
Race (4)
 Black, Non-Black Hispanic, Asian and Pacific Islander, Non-Hispanic Whites and Others
Place/Size (7)
 Central city of major metropolitan area, central city of other large metropolitan area, and so on
Housing Tenure (2)
 Owner, Nonowner
Age (6)
 0–9, 10–19, 20–29, 30–44, 45–64, 65 and over
Sex (2)
 Male, Female

In general, regional differences were preserved over differences between Blacks and Hispanics, and place/size differences were preserved over housing tenure. Asian and Pacific Islanders were combined with Non-Hispanic Whites for divisions without separate Asian post-strata. After combining to reduce the number of small cells, the first five criteria defined 116 post-strata groups, including a special group for American Indians living on reservations and trust land. The post-strata groups are listed in Appendix Table A.1. Each of the 116 post-strata groups was subdivided into the six age and two sex categories to produce 1,392 post-strata.

The dual-system model used to estimate the true population classifies each person as being either included or not in the Census enumeration, as well as being either included or not in the PES:

CENSUS ENUMERATION

PES	In	Out	Total
In	N_{11}	N_{12}	N_{1+}
Out	N_{21}	N_{22}	N_{2+}
Total	N_{+1}	N_{+2}	N_{++}

In theory, all cells are observable except for N_{22} and any of the totals that include N_{22}. The model assumes independence between inclusion in the census and the PES. This means that the probability of being in the ijth cell, p_{ij}, is the product of the marginal probabilities, $p_{i+}p_{+j}$. With this assumption, the estimate of the total population, N_{++}, is

$$N_{++} = (N_{+1})(N_{1+})/N_{11}.$$

This is called the *dual-system estimator* (DSE); see Wolter (1986).

To estimate the cells of the dual-system model, the PES conducted an independent listing of each sample block, an initial interview, an initial match to the census, a followup interview of problem cases, and a final match. The estimation steps included missing-data adjustment, weighting, and dual-system estimation. These steps are discussed in detail in the following sections.

After computing the dual-system estimates for all post-strata, the estimated population can be compared to the census count. The ratio of the PES estimate of the true population to the census count is called the adjustment factor. A regression smoothing model was used to reduce the variance of the factors. The results of this process were applied to the census figures to form a synthetic estimate at the lowest level of census geography, the block. The same post-strata were used for both the dual-system estimation and the synthetic distribution.

2.2 Sampling, Listing, and Interviewing

The primary sampling unit for the 1990 PES was the block cluster composed of either a block or a collection of blocks. A sample of 5,290 block clusters was chosen. The same blocks were sampled for both the P sample and the E sample. The P sample consisted of all people living in the sample blocks at the time of the PES interview. The E sample consisted of all census enumerations coded to the sample blocks, whether or not they actually belonged there. The PES sample excluded people living in institutions (jails, nursing homes), military personnel living in barracks or on ships, and people living in homeless shelters or on the street.

PES field work began before Census Day (April 1, 1990) when permanent Census Bureau staff visited each sample block to make a list of all housing units and group quarters. The PES household interviewing was scheduled to start in June; however, census nonresponse follow-up was still being conducted in many areas. Therefore, the PES interviewing had to be delayed, and the end of interviewing was shifted accordingly. PES interviewing was complete in most areas by the end of July and finished everywhere by early September. Interviewing was conducted mainly by temporary employees who had worked on the census enumeration. To increase the independence of the PES from the census, these employees were not allowed to work in areas that they had previously enumerated. During September, nonresponse cases were sent back to the field to be interviewed by permanent Census Bureau interviewers. This was done in all areas with an initial nonresponse rate of more than 2%. More than 3,700 cases were sent back, and 70% were converted to interviews.

Table 1 gives the results of P-sample interviewing. The final noninterview rate was less than 2%. But about 5% of the interviews were not with a member of the household and were considered to be of questionable quality.

Table 1. Initial Interviews by Outcome

	Total	Percent of occupied units
Total housing units	166,065	
Vacant	22,247	
Occupied units	143,818	100.0
Interviews		
Household member	134,808	93.7
Other	6,745	4.7
Noninterviews	2,265	1.6

2.3 Matching

For the purpose of the dual-system estimate, a person was considered enumerated by the census if his or her name was listed on a census record that was included as part of the population count. A person was considered omitted from the census if he or she should have been part of that count but was not. The matching rules classified persons as enumerated only if they were counted at the location where they should have been counted, according to the information they provided. For example, people who moved between April 1 and the end of census follow-up might be missed at their correct Census Day address but erroneously counted at their new address. The PES design would consider the people as missed by the census. The enumerations at the new address would be classified in the E sample as erroneous. In this example there would be both omissions and erroneous enumerations. If both addresses were in the same post-stratum, then the errors would tend to cancel.

An exact address match was not required. If a person reported that he lived at a given address, then the matching classified him as correctly enumerated if he was counted anywhere in the block where the address was located. It also classified him as correctly enumerated if he was counted in a ring of surrounding blocks. This ring of blocks whose census records were searched for a match was known as the search area. The search area was limited to one ring of adjacent blocks in urban areas and two rings in more rural areas. If a census operation coded the address outside the correct search area, then the matching counted the person as missed by the census. Census enumerations that were outside the search area of the true location were classified as erroneous, so that the overall estimate of net undercount would not be inflated.

Some cases lacked sufficient information to determine whether the person was enumerated. These cases were called "unresolved" and were imputed. Examples of P sample unresolved cases are records without names or interviews where the Census Day address is not reported.

The first stage of matching was done by a computer matching system that the Census Bureau had developed over the decade; see Jaro (1989). Computer matching used data on the individual characteristics and address information that the census routinely computerizes. In addition, to assist computer matching, the names of the people enumerated in the search area were keyed. The computer matching only worked for people who were living in the sample clusters and search areas on Census Day, because outside these areas the names were not keyed. Instead, clerks assigned the reported Census Day address to a census block using maps and computerized address-browsing programs. Copies of the census questionnaires were then searched by the clerks, who assigned a match code.

An initial match code was assigned to all cases before follow-up. These codes were used in the missing data imputation model to predict enumeration status for cases that could not be interviewed during follow-up. Most P-sample cases that were not matched were sent to the field for follow-up in late November and early December. Nonmatches where other household members matched were not sent to follow-up, provided that the initial information was reported by a household member. Not sending these nonmatches reduced the follow-up workload and allowed the limited pool of better-trained interviewers to concentrate on the other cases. After follow-up, clerks assigned a final match code. The final match codes provide important information for studying the nature of census errors beyond the question of net undercount.

2.4 Measuring Erroneous Enumerations

The E sample measures the proportion of erroneous census enumerations. The design considers an enumeration as correct if it is determined to not be a duplicate and if, according to the information provided, the person should have been counted either in the sample block or in one of the surrounding blocks that make up the search area. Erroneous enumerations include census duplicates, census fictitious enumerations, people who were born after Census Day or who died before Census Day, people counted in the wrong location, and census enumerations with insufficient information to allow both matching and follow-up reinterview.

An important category of erroneous enumerations were people who moved from outside the search area into the sample block after Census Day and were subsequently counted there in the census. All such people were considered to be erroneously enumerated. But under the search area concept, if they merely moved from one address within the search area to another, they were to be considered correctly enumerated so long as they were counted only once.

The PES used information gathered from the P sample to code the E sample, whenever records from the two samples were linked (matched). If someone in the P sample had indicated that he had not moved, the corresponding E-sample record was coded as correctly enumerated. Two records were created for movers. A P-sample record reflected their reported residence on Census Day. To facilitate computer matching for the E sample, the PES created another record at the sample address. If the mover was linked to a Census enumeration at the sample address, then the census record was to be treated as erroneously enumerated. The only exception was to be if both addresses were within the same search area. Unfortunately, errors occurred in applying these rules during computer edits. These are discussed in Section 3.

Census enumerations that were not linked to a person interviewed in the PES were sent to follow-up. The information gained during follow-up allowed the clerks to determine whether the enumeration referred to a real person and whether that person lived in the search area on Census Day.

The census included enumerations with such sparse data that they did not identify a unique individual. A common example is enumerations without names. Such cases could not be matched accurately to a P-sample case, nor could they be sent to follow-up to determine whether the person was real and lived there on Census Day. These enumerations are considered unmatchable and were counted as erroneous enumerations. Thus no P-sample cases were allowed to "match" these cases. The census count also includes whole-person imputations; that is, cases where the data about an individual were so sparse that another record was substituted. These cases were classified in the dual system estimation as not being in the census.

Some census cases were enumerated too late (November–December) to be included in the PES processing but are included in the census counts. These constituted .1% of the E sample for nonminorities and .4% for minorities. These cases introduce an upward bias into the dual-system estimate if they either should have matched or should have been classified as erroneous enumerations.

2.5 Estimation

P sample missing data occurred because of initial noninterviews or partial interviews and from failed or incomplete follow-up interviews. In the E sample noninterviews arose only from the follow-up because "nonresponse" census enumerations are treated as erroneously enumerated. Table 2 gives the level of missing enumeration status. The overall level is low, but, as expected, the pattern of PES response roughly parallels the pattern of census response.

Response cases were first reweighted to account for the whole-household noninterviews. Next, the missing data adjustment imputed any missing demographic characteristics so that each case could be assigned to a post-stratum. To account for unresolved enumeration status, a large logistic regression model was fit to P-sample data for which enumeration status was observed. This model was used to predict the probability of correctly enumerated versus that of omitted from the census for unresolved P-sample cases. A separate logistic regression model was fit to resolved E-sample individuals to predict the probability of correctly enumerated versus erroneously enumerated for unresolved E-sample cases, see Belin and Diffendal (1991) for details.

Dual-system estimates were made for each of the 1,392 post-strata, assuming independence of inclusion in the census and PES. Note that in the dual-system model, the marginal total, N_{+1}, is the number of distinct and identifiable people in the census. This differs from the official census count, which includes duplicates, fictitious cases, and other erroneous inclusions as well as imputations. The proportion of erroneous Census data-defined cases is measured by the E sample. Specifically, the estimator takes the following form within post-strata:

$$\begin{aligned}\hat{N}_{++} &= (N_{+1})(N_{1+})/N_{11}\\ &= (N_c - II)(1 - \widehat{EE}/\hat{N}_e)(\hat{N}_p/\hat{M}),\end{aligned}$$

where

$\hat{N}_{++}$ = dual-system estimate of the population,
$\hat{N}_p$ = weighted P-sample total $(=N_{1+})$,
N_c = census count,
II = number of whole-person census imputations,
$\widehat{EE}$ = weighted estimate of E-sample erroneous enumerations,
$\hat{N}_e$ = weighted E-sample total,
$\hat{M}$ = weighted estimate of P-sample matches $(=N_{11})$.

Note that

$$N_{+1} = (N_c - II)(1 - \widehat{EE}/\hat{N}_e).$$

When we computed the DSE's, we noted that two block clusters exerted extremely large leverage on the estimates. Leverage was defined as the absolute value of the difference between the weighted number of nonmatches and the weighted number of erroneous enumerations for the cluster. Both of these block clusters were drawn from a special sample of census blocks where few housing units were expected and low sampling probabilities (and corresponding high sample weights) applied. The possibility of such cluster outliers had been anticipated; accordingly, both block clusters were downweighted, and the DSE's were recomputed.

2.6 Smoothing and Synthetic Estimation

The difference between the estimated population $\hat{N}_{++}$ and the census count N_c (without removing imputations or erroneous enumerations) estimates the net census undercount. The ratio of the estimated true population $\hat{N}_{++}$ to the census count N_c is the adjustment factor.

It was anticipated that many of the 1,392 post-strata adjustment factors would have variances too high for them to be useful for adjustment. One way to reduce the variance would be to form fewer post-strata; that is, to assume homogeneity across broader categories. This approach is discussed in Section 3. Instead, for the census adjustment estimates, a regression smoothing approach was adopted. A regression model was fitted to predict the adjustment post-stratum factors in a way that allowed for sampling error. The regression-predicted factor was then "averaged" with the observed factor to form the smoothed factor. The model thus attempted to "borrow strength" from many cells, somewhat in the spirit of a Bayes estimation approach. In more detail, the model was

$$\mathbf{Y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{w} + \mathbf{e}, \qquad (1)$$

where

$\mathbf{Y}$ = observed adjustment factor by post-stratum,
$\mathbf{X}$ = matrix of carrier (regression) variables,
$\boldsymbol{\beta}$ = vector of fixed effects (regression parameters),
$\mathbf{w}$ = model error, assumed $N(\mathbf{0}, \sigma^2\mathbf{I})$,
$\mathbf{e}$ = sampling error, assumed $N(\mathbf{0}, \mathbf{V})$, where $\mathbf{V}$ is the sampling error covariance matrix.

Table 2 Percent Unresolved by Race/Ethnic Group

	P sample	*E sample*
Non-Hispanic White and Other	1.6	.7
Black	2.5	2.1
Hispanic	2.5	1.8
Asian and Pacific Islander	2.0	1.3

The observations were the adjustment factors for the 1,392 post-strata. The model was fit separately for the four census regions, and a reduced model was used for the special American Indian strata.

The variables used to form the post-strata were also used as predictors. These variables were expressed as indicators. If categories were combined, then the variables were expressed as proportions. For example, when Blacks and Hispanics were combined in one post-stratum, the "Black" indicator would be the proportion black in that post-stratum and the "Hispanic" indicator would be its complement. There were indicators for race and Hispanic origin, age category, tenure, census division, and place/size category. Interactions were allowed between race and place/size, among age-sex-race, and among age-sex-tenure. Other variables measured the difficulty in taking the census. These included the proportion of people enumerated on questionnaires returned by mail and the proportion of census whole-person imputations. Another variable indicated the proportion of enumeration conducted using traditional door-to-door enumeration, a method used primarily in remote rural areas.

Indicators for race, age, and tenure were forced to enter the model, with the other variables selected based on their predictive power. The carrier variables were selected using a best-subsets regression (Furnival and Wilson 1974). This approach was chosen over more subjective approaches to meet the requirement of prespecification.

Experience from earlier tests and theoretical considerations suggested that the estimated sample variances would be higher for large or very small estimated adjustment factors. This was indeed the case (see Fig. 1). If the estimated sample variances were related only to the true adjustment factors, then this dependence would have been appropriately accounted for in the generalized least squares fitting of the model and subsequent smoothing. But it was likely that the sampling errors of the estimated variances were positively

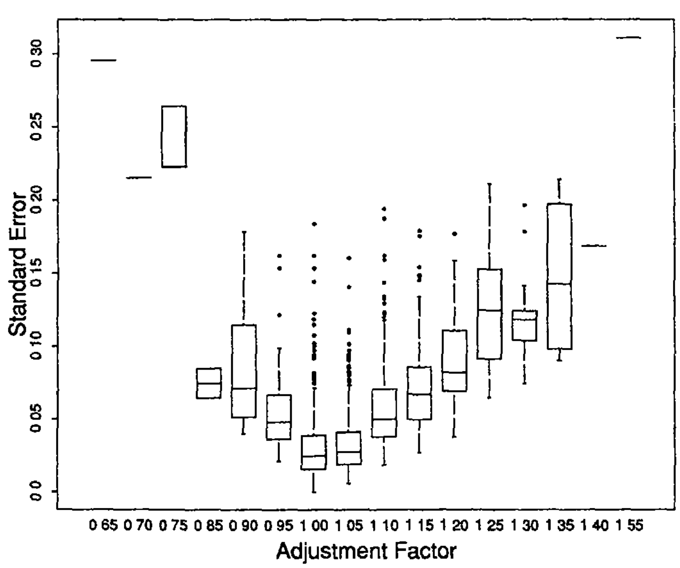

Figure 1. Estimated Standard Errors versus Estimated Adjustment Factors. Each boxplot shows the distribution of estimated standard errors for a .5 range of estimated adjustment factors. Labels indicate center of the range. The data represent 1,392 factors from original adjustment estimates

correlated with the sampling errors of the estimated adjustment factors. This could have resulted in underweighting or overweighting of certain factors. For this reason, and also in an effort to "borrow strength" to improve the sampling error variance estimate, the post-stratum variances were presmoothed using the model

$$nv/(1 + C^2) = b_0 + b_1W + b_2A_1 + b_3A_2 + b_4M + \text{error}, \quad (2)$$

where

v = true variance of the raw adjustment factor,
n = P-sample number of people in the post-stratum,
C = coefficient of variation of the P-sample person weights,
W = a regression approximation to the adjustment factor, constrained to be at least 1.00,
A_1 = age indicator for ages 0–19,
A_2 = age indicator for ages 20–44,
M = variable indicating the proportion of minority in the post-strata.

The term "W" was included to account for the correlation between the true variance and the true adjustment factor. It was estimated using the same carrier variables and best-subsets regression program as for (1) but using the sample estimated variances.

The variance smoothing model seemed to succeed in pulling up the low estimated variances. But for some points with high sample variances, the model (2) predicted much lower variances, suggesting that these points were outliers in regard to (2). Using these model variances in fitting (1) would have given these extreme points much greater weight than that achieved using the raw variances. This problem had not been anticipated, and no solution had been prespecified. To lessen this problem, any point with a studentized residual greater than 4 was omitted from modeling the variances with (2) and the raw sample estimated variance was used for such points in fitting (1). Two iterations were used to identify outliers. The original correlations were used with the presmoothed variances to produce a "smoothed" covariance matrix.

The smoothed adjustment factors were computed by "averaging" the predicted and sample estimated adjustment factors,

$$\hat{\mathbf{y}} = \mathbf{X}\hat{\beta} + \hat{\sigma}^2\mathbf{I}\hat{\Sigma}^{-1}(\mathbf{Y} - \mathbf{X}\hat{\beta}),$$

where $\hat{\mathbf{y}} = \mathbf{X}\hat{\beta} + \mathbf{w}$ is the true adjustment factor and $\hat{\Sigma} = (\hat{\mathbf{V}} + \hat{\sigma}^2\mathbf{I})$. If there were no covariances (if $\mathbf{V}$ were diagonal), then the method used would add back to the regression estimate a part of the residual proportional to the model variance and inversely proportional to the sampling variance. Because $\mathbf{V}$ was not diagonal, the actual smoothed factors, $\hat{\mathbf{y}}$, sometimes fell outside the interval between the observed and regression adjustment factors. As a final step, the smoothed factors were ratio-adjusted so that for each region, the smoothed undercount equalled the directly estimated undercount. A further description of the smoothing process was provided by Isaki, Huang, and Tsay (1991).

The census adjustment estimates showed a net national undercount of 2.1%. Higher undercounts were measured in the South and West and lower undercounts in the Northeast and Midwest. The levels and patterns of the measured undercounts generally followed expectations. Undercounts tended to be higher for Blacks, Hispanics, and Asians and were high for nearly all nonowner post-strata groups. Undercounts tended to be low in suburban areas and small towns.

Smoothing produced a curious result for the South. The unsmoothed estimates were highest in the South Atlantic (3.5%), whereas the undercount rates for the East South Central and West South Central were much lower: 1.2% and 2.1%. Each had an estimated standard error of .6%. Smoothing brought all three divisions together, with the West South Central now being slightly higher. That is, smoothing lowered the undercount for the South Atlantic to 2.6% and raised the undercount to 2.4% in the East South Central and 2.9% in the West South Central Divisions.

Appendix Table A.1 gives the unsmoothed and smoothed results from the Census adjustment process for the 116 post-strata groups; that is, summing across age and sex. This table allows one to see both what the PES measured and the effects of smoothing on the results. Several post-strata groups were of special concern. In New England Central Cities and in Other (rural) areas in the East North Central, the PES measured a large overcount. The smoothing reduced but did not eliminate the overcount. Due to the limited sample sizes for Hispanics in the Northeast Region, there were only two separate Hispanic post-strata groups, with an additional three groups where Hispanics were combined with Blacks. The two separate groups were New York City and Central Cities of other large metropolitan areas, with unsmoothed estimates of 4.0% and 9.9%. Both were estimated with high standard errors: 3.8 and 6.1. The smoothing reduced these estimated undercounts to 1.7 and 2.0—below the national average. The original unsmoothed estimate for Blacks in Pacific Non-Central-City areas of the Metropolitan Statistical Area (MSA) was 14.3% among the highest measured. Smoothing raised this estimate to 16.4%.

Distribution of the estimated undercount geographically below the post-stratum level was done by multiplying the post-strata adjustment factors by census counts for each post-stratum in each block in the census. The block level was used to ensure that all subsequent tabulations based on the adjustment were consistent. The census counts for groups excluded from the PES frame (e.g., the institutional population) remained unchanged.

The process generally did not produce whole numbers of persons. Fractions were rounded either up or down to a whole person, using a controlled rounding procedure that ensured that the post-strata within a block as well as the total for any block were not rounded up or down by more than 1. The totals by post-strata for states were controlled to the level of precision of the computer, roughly 10 people. To reflect the count adjustment in individual records for the census blocks, whole-person records were imputed using a procedure similar to that used by the census for other missing data. For overcounts, a "negative" record was imputed.

Had the census been adjusted based on the PES results, the official count of the resident population would have increased by 5.27 million. That would have made the official resident population of the United States just under 254 million. Of the increase, 1.5 million (29%) would have been Black, 1.2 million (23%) would have been Hispanic, 231,000 (4.0%) would have been Asian and Pacific Islander, and 99,000 (1.9%) would have been American Indian. The rest would have been Non-Hispanic Whites and Others. These are net numbers. In fact, 6.19 million records would have been added to account for net undercount and 919,000 records "subtracted" to account for net overcount. After the count adjustment record was imputed, the adjusted files were tabulated. These were completed in time for the July 15 deadline.

3. IMPROVING THE ESTIMATES

There were several criticisms of the approach used in the July census adjustment estimates (U.S. Department of Commerce 1991). In terms of producing alternative undercount estimates for use in the postcensal estimates program, these criticisms were paramount:

1. The use of the smoothing models led to estimates whose true uncertainty was difficult to assess. For example, smoothing assumed that the variance matrix (**V**) was known with certainty, when it was actually estimated. The effects of uncertainty of the estimated covariances were of special concern.
2. The post-strata were possibly too heterogeneous, especially geographically, to be suitable for the synthetic estimation of undercount for small areas. For example, Delaware is included in the South region rather than the Northeast.
3. The direct (unsmoothed) estimates were thought to be biased. The biases of a PES are well documented (see Hogan and Wolter 1988; Mulry and Spencer 1991). Not all errors could be corrected. Reduction in the bias due to misreporting of Census Day address was unlikely to be corrected over a year after Census Day. Other biases might be reduced, including matching bias.

Two approaches were taken to respond to these criticisms. First, new post-strata were developed. This step was designed to increase homogeneity by forming better post-strata and at the same time to decrease the variance of the direct dual-system estimates by forming fewer post-strata. Second, the basic PES data set was modified to remove some of the bias and to reduce variance.

The alternative post-stratification was designed to produce estimates of the relative proportions by state and local areas that did not rely on statistical smoothing. In forming post-strata, one is faced with two opposing goals. First, one would like each of the post-strata to be as homogeneous as possible. This can be accomplished most easily by forming many, relatively small post-strata. But in general, for any fixed overall sample size, more post-strata mean smaller sample sizes within each and so higher variance for each of the post-strata. Of course, not only the variance of the post-strata is important, but also the variance of the state and local estimates produced. Large post-stratum variances can lead to large variances for estimates of interest. Because the goal was to develop fewer as well as more homogeneous post-strata, it was important to choose the stratification variables wisely.

The original 116 post-strata groups were developed before the census and the PES were conducted. In forming the new post-strata, the results of both the PES and the census were known. The original 116 post-strata had been based on a hierarchy in which geographic differences were largely preserved over race and ethnic differences. Differences in place/size were preserved over differences in housing tenure. The results of the PES did not necessarily validate this hierarchy. For example, differences between some place/size categories were often small, whereas differences between owners and renters were often striking. Divisional differences showed a confusing pattern often obscured by high variances.

In developing the new post-strata, there was a limit to the extent that the PES results could be used directly. First, because the results were subject to (sometimes quite high) variances, combining groups with similar estimated undercounts was not exactly the same as combining groups with similar true undercounts. (General patterns as revealed from statistical analysis did prove useful, however.) Further, the existing post-strata groups could not be used to suggest completely new groupings. For example, they could not help in determining whether a different measure of "urbanization" might be superior to the measure originally used.

Instead, the analysis focused on measures of census performance derived from the complete census file, such as mail-return rates and whole-person substitution rates. Measures of crowding, proportion of nonhousehold members, item imputation rates, and a few other variables also proved helpful. The working assumption was that post-strata defined to be relatively homogeneous with respect to these variables would also be relatively homogeneous with respect to the undercount. The results of this analysis suggested a hierarchy of:

Race (4)
  Black, Non-Black Hispanic, Asian and Pacific Islander, and Non-Hispanic White and Other
Housing Tenure (2)
  Owner, Nonowner
Urbanization (3)
  Urbanized areas with population greater than 250,000
  Other urbanized and urban areas
  Rural
Region (4)
  Northeast, South, Midwest, West

The separate group for American Indians on reservations was maintained. Considerable research went into deciding whether there was a grouping of states or even counties that was better than the four traditional Census regions. Although some alternative patterns did emerge, none were consistent across the variables of interest (i.e., mail back rate, allocation rate, and so forth). The decision was made to continue to use the traditional four Census regions because of their familiarity to users of Census products, but to drop the finer breakdown by divisions.

Each new post-strata group was divided by age and sex into estimation post-strata. The census adjustment estimates used 12 age-sex groups: that is, 6 age groups cross-classified by the two sexes. This scheme had several drawbacks. Most important, it produced far too many cells (1,392). One-quarter of the original cells contained fewer than 130 P sample cases; the smallest contained only 8. Research conducted over the summer with the original 116 post-strata groups but with only 6 age-sex groups showed great advantage over the original scheme (Hogan and Isaki 1991).

From a demographic standpoint, the 6 age groups were not well chosen. The most glaring problem concerns the group for ages 10–19. It seems clear that in terms of life-style (mobility, independence, and so forth) that a 19-year-old has little in common with a 10-year-old. This suggested an alternative age grouping: 0–17, 18–29, 30–49, and 50 and over. Finally, there seemed to be no reason to calculate separate estimates for girls and boys 0–17. Demographic analysis had never shown a sex difference for this group. The census adjustment estimates had shown little difference in undercount between these groups. Therefore, rather than the original 12 age-sex groups, the postcensal post-strata have only 7.

The revisions gave 357 post-strata rather than 1,392. The restratification was most successful in avoiding the very small sample sizes, which had led to high variances and difficulties in estimating the covariance matrix. Small post-strata also led to ratio-estimation bias, which could also be reduced with the new post-stratification.

Since the time that census adjustment files were produced, the PES data file has been modified in several ways to reduce some of the biases and variances. New clerical matching has been conducted on a set of blocks and several computer edits have been applied to the data file.

The 104 block clusters with the highest leverage on the PES estimates were reworked by a group of matching experts. The measure of leverage is the same as that used to downweight the outlier clusters. The matching staff reviewed all aspects of the matching for these block clusters. In general, they applied the same matching rules as were to be used in the production (i.e., November 1990 to January 1991) matching. For example, they determined correct Census Day addresses and searched for new matches (P sample) as well as new duplicates (E sample).

In a few cases the matching rules were modified. Of particular interest was the definition of search area. The original definition of search area was the sample block and either one ring (in urban areas) or two rings (in more rural areas) of surrounding blocks. If applied consistently to both the P sample and E sample, this rule will produce unbiased estimates (in the absence of other errors). Unfortunately, it can also produce estimates with extremely high variances. If the census incorrectly assigns (misgeocodes) a large group of housing units just outside the search area, then the rule will produce either a high number of nonmatches or a high number of erroneous enumerations, depending on which blocks fell in the sample. Over all possible samples, the estimator will balance. But because misassigning large numbers of housing units is a rare event, for any actual sample one will usually observe either a high number of nonmatches (high measured undercount) or a high number of erroneous enumerations (high measured overcount).

In the census adjustment estimates, these effects were smoothed out over an entire region. Without smoothing, the

Table 3. Undercounts by Race/Ethnic Origin and Tenure

	Total	Owner	Non-owner
Non-Hispanic White and Other	7	− 3	3 1
Black	4 6	2 3	6 5
Hispanic	5 0	1.8	7 4
Asian and Pacific Islander	2.4	−1.4	7.0
Reservation Indian	12 2	n/a	n/a

entire effect would be left within a particular post-stratum. Therefore, in the rematching the search area was sometimes expanded by an extra ring if it seemed that strict application of the production rule was the main cause of a cluster's high influence.

A problem was discovered in the computer editing of erroneous enumerations. The PES was designed to treat as erroneously enumerated any person counted by the Census in the sample block who did not usually reside in the sample block or the search area on Census Day. The PES used information gathered from the P sample to code the E sample, whenever records from the two samples were linked (matched). If a mover was linked to a Census enumeration at the sample address, then the census record was to be treated as erroneously enumerated.

Unfortunately, an error occurred in carrying out this step in the census adjustment estimates. Essentially, the edit was applied to the E sample only if the P-sample Census Day ("mover") record was matched. Otherwise, the E sample record was treated as a correct enumeration. Thus more than 2,000 E sample matches to in-movers that should have been treated as erroneously enumerated were treated as correctly enumerated in the July 1991 PES estimates. Additionally, the edit was incorrectly applied to only part of the entire file. Because of this, more than 560 cases were coded as erroneous enumerations when they should have stayed as matches. Correcting all of these records lowered the estimated net undercount by about .4%. In addition, there were a few other rather minor edit corrections, as well as a small improvement to the missing data imputation program.

4. UNDERCOUNT RATES FOR USE IN THE POSTCENSAL ESTIMATES

4.1 Net Coverage Error

The net result of the work done since July has been to lower the PES estimates of the undercount by about $\frac{1}{2}$ of a percentage point, from around 2.1% to about 1.6% (with a standard error equal to .2 for each estimate). This reduction tends to bring PES estimates at the national level more in line with the 1.8% undercount estimated by demographic analysis comparison to vital records and other independent data sources. The original production estimates would have added 6.19 million records while "subtracting" 919,000. The new estimate adds 5.45 million records (a decrease of $\frac{3}{4}$ million); however, 1.46 million records are now "subtracted" (an increase of $\frac{1}{2}$ million). Again, the important issue is the pattern of undercount by area and group.

Table 4. Estimates for Revised Post-Strata Groups

	Percent undercount					Standard errors				
	All	NE	S	MW	W	All	NE	S	MW	W
Non-Hispanic White and Other										
Owner										
Large Urbanized Areas		−2 13	68	−.26	−.34		1.08	71	39	.65
Other Urban		−1.08	.52	−.10	62		.49	.42	40	58
Non-Urban		−.54	.18	−.71	29		70	.69	1.18	69
Nonowner										
Large Urbanized Areas		1 16	2 56	2.33	3 18		1 39	1 48	1.61	1 62
Other Urban		3.41	3 20	1 23	4.49		1 51	1 74	1 09	1 34
Non-Urban		6.52	6 23	2 85	6 08		4 20	1 71	1 51	1 81
Black										
Owner										
Large Urbanized Areas		1 63	2 16	81	6.10		1 91	.90	.87	1.91
Other Urban	1.34					.98				
Non-Urban	3.52					1 90				
Nonowner										
Large Urbanized Areas		8 37	6 27	5.99	9.96		1.61	1.90	1 68	2 72
Other Urban	4 15					1 18				
Non-Urban	4 62					5 33				
Non-Black Hispanic										
Owner										
Large Urbanized Areas		67	2.53	−4.33	2.89		4 45	90	2 58	87
Other Urban	.94					1 64				
Non-Urban	2 73					2 69				
Nonowner										
Large Urbanized Areas		6.72	9.34	6.64	5.91		3 51	2.59	3 26	1 84
Other Urban	6.60					2.74				
Non-Urban	15.80					5 01				
Asian and Pacific Islander										
Owner	−1.45					1 50				
Nonowner	6.96					2 52				
Reservation Indians	12.22					4.73				

Table 3 gives the corrected results by race and tenure. The undercount for Non-Hispanic Whites and Others is relatively low (less than 1%), whereas the undercount for Blacks and Hispanics is relatively high (4.5% to 5.0%). The undercount rate estimated for Asians lies in between. The results seem to show that tenure is as important as race in explaining undercount. This result, if supported by other research, has important implications for planning not just the postcensal estimates but also the next census. The spread between Asian owners and Asian nonowners is much larger than for other groups. This is probably because Asian nonowners tend to be disproportionately recent immigrants, whereas Asian owners are drawn from more established communities. At this time, we can only speculate as to whether the difference between tenure groups is because of tenure itself (i.e., renters tend to move more often) or because owners and renters are drawn from different groups.

Table 4 gives the new undercount estimates for the revised post-strata groups, together with their estimated standard errors. These are the key results for use in the postcensal estimates. The patterns by race and tenure are evident, as is a regional pattern, with the undercounts for the West and South being somewhat higher than those for the Northeast and Midwest. The Non-Urban areas often have higher estimated undercounts than do the Urban areas, but they also often have high standard errors, which makes interpretation difficult.

A few cells are of particular interest. The estimates for non-Hispanic Whites in both Large Urban and Other Urban areas in the Northeast are negative; that is, they have overcounts of 2.1 (standard error = 1.1) and 1.1 (.5). These are on the margin of significance at the 5% level. These numbers are applied to very large groups, which together comprise approximately 20 million people, and produced an estimated overcount of 376,000. Still, comparing these cells to nearby cells does not seem to show that these estimates are far out of line.

The estimates for the individual 357 post-strata tend to show the same general pattern, of course, but are much less stable. Figure 2 shows the distribution of post-strata estimates by race and tenure group. The viewer can easily see that even

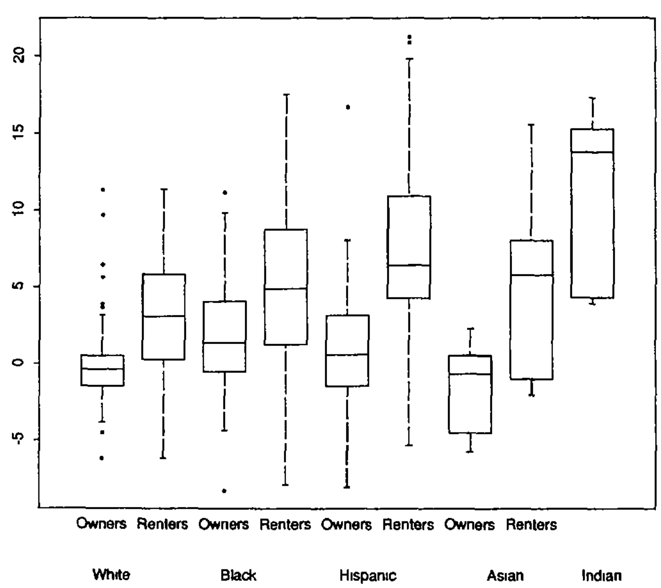

Figure 2. Distribution of Undercount Rates by Race and Tenure. Each boxplot shows the distribution of the estimated undercount rates in percent for the five race groups and tenure status. Data points are the 357 undercounts for the revised post-strata

Table 5. Counts and Undercount Rates by State: Adjustment and Revised

Name	Census count*	Adjustment estimate Count*	Rate	SE	Revised estimate Count*	Rate	SE
Alabama	4,041	4,146	2.5	.4	4,113	1.8	.3
Alaska	550	561	1.9	.4	561	2 0	4
Arizona	3,665	3,790	3 3	5	3,754	2 4	5
Arkansas	2,351	2,403	2 2	4	2,392	1.7	3
California	29,760	30,888	3 7	4	30,595	2.7	.4
Colorado	3,294	3,376	2 4	.5	3,364	2.1	.4
Connecticut	3,287	3,306	.6	.6	3,308	6	4
Delaware	666	687	3.0	.4	678	1.8	4
DC	607	639	5 0	.5	628	3 4	9
Florida	12,938	13,278	2.6	.4	13,197	2.0	.4
Georgia	6,478	6,633	2.3	.4	6,619	2.1	.3
Hawaii	1,108	1,136	2 5	.5	1,129	1 9	8
Idaho	1,007	1,035	2.8	.5	1,029	2 2	.4
Illinois	11,431	11,592	1.4	.4	11,544	1.0	.4
Indiana	5,544	5,586	7	4	5,572	5	.4
Iowa	2,777	2,807	1.1	.5	2,788	.4	.4
Kansas	2,478	2,506	1.2	.4	2,495	7	4
Kentucky	3,685	3,768	2.2	.4	3,746	1 6	4
Louisiana	4,220	4,332	2.6	.4	4,314	2 2	4
Maine	1,228	1,240	1 0	6	1,237	.7	6
Maryland	4,781	4,869	1.8	.4	4,882	2.1	.4
Massachusetts	6,016	6,039	4	5	6,045	.5	.5
Michigan	9,295	9,404	1 2	.4	9,361	.7	4
Minnesota	4,375	4,419	1.0	4	4,394	.4	.4
Mississippi	2,573	2,632	2.2	.4	2,629	2.1	.4
Missouri	5,117	5,184	1 3	4	5,149	.6	.4
Montana	799	822	2 8	5	818	2.4	.5
Nebraska	1,578	1,595	1 0	4	1,589	.6	.4
Nevada	1,202	1,232	2.4	.5	1,231	2 3	.4
New Hampshire	1,109	1,116	.6	5	1,119	8	.5
New Jersey	7,730	7,836	1.4	5	7,774	6	6
New Mexico	1,515	1,586	4.5	5	1,563	3.1	.5
New York	17,990	18,304	1 7	5	18,262	1.5	6
North Carolina	6,629	6,815	2 7	4	6,753	1 8	.3
North Dakota	639	648	1.4	.5	643	.7	.5
Ohio	10,847	10,933	.8	.4	10,922	7	4
Oklahoma	3,146	3,214	2.1	4	3,203	1 8	3
Oregon	2,842	2,898	1 9	.4	2,896	1.9	4
Pennsylvania	11,882	11,957	6	5	11,917	.3	.5
Rhode Island	1,003	1,006	3	6	1,005	1	.6
South Carolina	3,487	3,590	2.9	.4	3,559	2 0	4
South Dakota	696	707	1.5	.5	703	1 0	.5
Tennessee	4,877	5,012	2 7	4	4,964	1 7	3
Texas	16,987	17,551	3 2	4	17,470	2 8	4
Utah	1,723	1,757	1 9	5	1,753	1.7	5
Vermont	563	571	1 4	.7	569	1.1	.8
Virginia	6,187	6,353	2.6	4	6,314	2 0	4
Washington	4,867	4,987	2.4	.4	4,958	1 8	4
West Virginia	1,793	1,842	2 6	4	1,819	1 4	4
Wisconsin	4,892	4,924	.7	4	4,922	6	4
Wyoming	454	466	2 7	5	464	2.2	4

* All counts in thousands

with fewer post-strata, the directly estimated undercounts are still widely dispersed.

One of the main interests in the undercounts in terms of the postcensal estimates is for the individual states. Table 5 gives the state census count, the production (July 15) state estimate (count, undercount rate, and standard error), and the revised state estimates. The estimated undercount is reduced for all but five states. It falls by more than 1% in Tennessee, Delaware, West Virginia, New Mexico, and the District of Columbia. The estimated population of California is reduced by over a $\frac{1}{4}$ million. At the other extreme, Maryland's estimated undercount rose by .3% and New Hampshire's increased by .2%.

Of particular interest is the estimate of relative proportions. Specifically, one can calculate the *relative* state undercount; that is, $100*(1 - pc_i/pa_i)$, where pc_i is the proportion of total population for state i in the Census and pa_i is its proportion in the adjusted population. Figure 3 plots these relative state undercounts for the Census adjustment figures versus the revised figures. They are clearly highly related, as should be expected. Their correlation coefficient is .93. Still, there are important differences between the two series; for example, the most extreme relative undercounts have been brought down by the new estimates.

4.2 Gross Census Errors

The PES was designed to measure the net undercount by group and to provide the data to adjust for that net undercount. It also provides data on the gross Census errors: gross omissions and gross erroneous inclusions. But one must take care in interpreting these data; some of the measures and concepts are appropriate only when considered in terms of the way they produce net estimates. In addition, all of these data are subject to sampling error, which for some groups and categories is quite large.

The PES estimates the proportion of the population not enumerated at their correct Census Day residence. Table 6

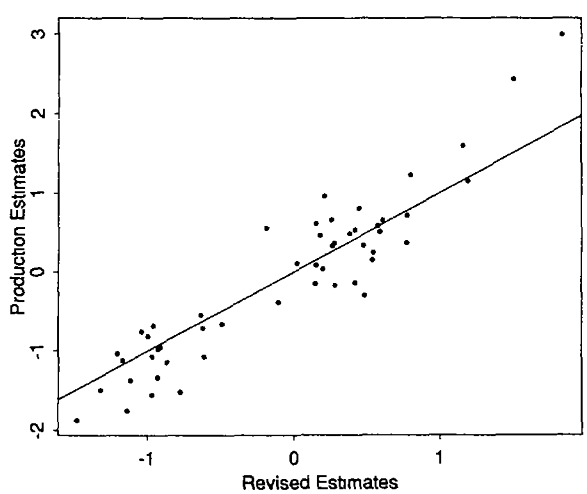

Figure 3. Percent Relative State Undercount Production and Revised Estimates Data are for 50 states plus District of Columbia Data are 1 minus the ratio of the proportion of U S total population for each state in the Census to the estimated true proportion, displayed as a percentage The reference line is at 45°

Table 6. Types of Nonmatches as Percent of Total Resolved Cases

	N1	N2	N3	N4	L
Total	1.8	2.0	5	1.3	.3
Non-Hispanic White and Other	1.3	1.6	3	1.3	2
Black	4.3	4.7	1.2	1.3	4
Hispanic	3.3	2.7	1.4	1.6	1.0
Asian and Pacific Islander	2.6	3.0	.8	6	5

gives the distribution of nonmatches by category (for nonmovers):

$N1$ = Nonmatched person within a household where other people matched

$N2$ = Nonmatched person within a household where no other person matched; however, the housing unit included in the Census

$N3$ = Nonmatched person within a missed housing unit; however, other housing units in building were included in the census

$N4$ = Nonmatched person living in a building missed by the census

L = Census processing error (i.e., person listed on a census questionnaire that was returned but not counted in the census.)

Several features are interesting. First, the PES nonmatches include a high proportion of within-household omissions. The next feature is the high number of $N2$'s, missed households within enumerated housing units. A missed household within enumerated housing units can happen in different ways. The housing unit could be enumerated as vacant. Another family in the building may have been enumerated in place of the missed household, as sometimes happens in older buildings without clearly marked apartment numbers. The enumerator may have created a fictitious household as a replacement. Another way would be if the enumerator failed to get a complete interview, causing the family to be either imputed in the census or classified as "unmatchable." Each of the last three ways would create an erroneous enumeration that would to some extent offset the omissions.

The revised PES data show some 14 million census erroneous enumerations, which together with 2 million census imputations are subtracted from the census counts before applying the DSE. How should one interpret this number? Table 7 gives the weighted distribution of erroneous enumerations by type. Some 28% are census duplicates. Under most definitions, these would be considered erroneous. About 2.6% are estimated fictitious, again clearly erroneous.

The PES estimated that about 6% of the erroneous enumerations were people who were enumerated outside the search area; that is, two or more blocks away. The block counts are clearly off, but if these persons were *missed* in the correct block (which we do not know), then as blocks are aggregated, the coverage errors cancel.

Most of the "other counting errors" are enumerations of people who moved into the address after Census Day. If they were missed at the correct location, then this may be the only place that they were enumerated. This type of error is often, but not always, paired with census omissions of the actual Census Day residents.

The "unmatchable" cases represent census enumerations without names. The PES required sufficient identifying information so that these persons could be matched or followed up. Without this information, they were coded "unmatchable." Many of these enumerations refer to real people who actually lived at the address, although others may be duplicates, fictitious, and so forth. The PES gives no direct information. Finally, the PES imputed roughly $\frac{1}{2}$ million erroneous enumerations. The imputation program only predicts a probability of the enumeration being erroneous. Summing these probabilities gives an estimate of the number but no indication of the probable cause.

Figure 4 plots the percent nonmatches against the percent erroneously enumerated and unmatchable in the census for the 51 post-strata groups. It includes a reference line plotted with slope 2.0 and intercept of −3%. Some of the variability observed here is due to sampling; however, we can see how much the net undercount measured by the PES is really a function of both nonmatches and erroneous enumerations.

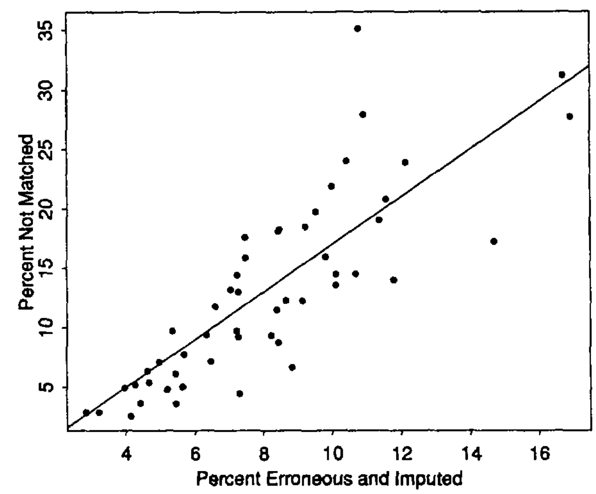

Figure 4. Percent Not Matched versus Percent Erroneous and Imputed. Data are for the 51 revised post-strata groups. The reference line is drawn with slope 2 and intercept at −3.

5. CONCLUSION

Perhaps it is still too soon to reach a conclusion about the 1990 PES. The litigation continues over the census adjustment estimates. The revised estimates are still being evaluated and analyzed in terms of bias and variance. Statisticians still hold different viewpoints, as is evidenced by this special section. Some results are now clear, however.

At a purely operational level, the PES succeeded. Completing all the operations necessary for census adjustment by July 15, 1991, was a monumental accomplishment. Adjusted census files were ready by the deadline. This success demonstrates the operational feasibility of census adjustment. Still, if similar methodology were to be used in the future, the time frame probably would have to be shortened even more to produce adjusted state numbers by December 2000.

Table 7. Distribution of Measured Erroneous Enumerations by Type

	Percent of total	Percent of EE's
Total EE	5.8	
Duplicate	1.6	28 2
Fictitious	2	2.6
Geocoding error	.3	6.0
Other counting errors	2 2	38 0
Unmatchable	1.2	20 8
Imputed EE's	3	4.5

Some of the revisions made since the census adjustment estimates were produced point the way to improvements for the future. The revised post-stratification seems to be a great improvement, producing more stable estimates with a sharper distinction between groups. This change reduces the need for statistical smoothing, but at the same time may make a smoothing process work better.

The PES is also serving its older function, as a tool of census evaluation. Understanding the importance of housing tenure in undercount will surely help guide future census outreach activities. Tracing the erroneous enumerations back to the census operation that produced them will help guide the design of future operations. This work has already begun. The PES can also inform the data users. Press coverage and anecdotal evidence often gave the impression that the 1990 census was an overall disaster. The results of the PES dispel this impression by putting the undercount in perspective. They allow each user to judge the probable effect of coverage error on a particular use of the data. This, of course, has long been the motivation for conducting coverage evaluation studies.

Appendix: Table A.1. Adjustment Dual-System Estimates

Percent Undercount by Post-Stratum Group

	Direct				Smoothed			
North East	All Other	Black	Hispanic	Asian	All Other	Black	Hispanic	Asian
New England								
Central Cities	-1.74	5.69			-1.16	4.25		
Non Central City MSA	0.61				0.19			
Other Places 10,000+	0.54	5.88 *			0.59	5.39 *		
Other areas	1.68				1.79			
Middle Atlantic								
New York City CC's								
Non-owner	2.06	6.44	4.00	9.47	0.87	7.76	1.73	10.50
Owner	-2.64	-2.86			-0.23	-0.15		
Other Large MSA Central city								
Non-owner	-6.41	10.78	9.91		-0.37	7.74	2.01	
Owner	-2.93	2.66			-0.19	-0.03		
Central cities of Small MSA	2.05	17.92			0.07	9.34		
Non Central City in NYC PMSA	5.03	5.63			0.42	6.73		
Non Central City in Other Large MSA	-0.80				0.36			
Non Central City in Small MSA	-0.78	5.88 *			-0.09	5.39 *		
Other Places 10,000+	1.36				0.41			
Other areas	0.43				0.70			
South								
South Atlantic								
Large MSA Central city								
Non-owner	11.49	10.46			5.00	9.33		
Owner	1.09	1.68	2.77		1.72	0.95	4.92	
Central cities of Small MSA	2.84	4.93			2.74	4.00		
Non Central City in Large MSA	0.93	4.17	13.79		0.44	1.97	5.13	
Non Central City in Small MSA	3.50	0.27			2.80	3.59		
Other Places 10,000+	1.23	-1.71			1.51	1.60		
Other areas	3.25	5.68			2.71	2.64		
East South Central								
Large MSA Central city								
Non-owner	2.17	6.46			4.80	5.81		
Owner	3.19				2.56			
Central cities of Small MSA	0.90				2.58			
Non Central City in MSA	1.42	4.82			2.31	2.26		
Other Places 10,000+	-6.02				1.84			
Other areas	-0.95				1.65			
West South Central								
Houston,Dallas, Ft.Worth CC's								
Non-owner	6.24				4.60			
Owner	0.56	8.09	8.96		1.49	6.64	7.11	
Other Large MSA Central city								
Non-owner	1.34				3.23			
Owner	-1.16	4.54	3.18		0.69	4.82	3.76	
Central cities of Small MSA	-3.16				2.48			
Non Central City in MSA	2.07				2.28			
Other Places 10,000+	1.19	1.66	2.36		1.25	2.28	5.11	
Other areas	1.72				1.96			

	Direct				Smoothed			
Midwest	All Other	Black	Hispanic	Asian	All Other	Black	Hispanic	Asian
East North Central								
Chicago Detroit CC's								
Non-owner	2.76	**6.76**	0.38		**5.17**	**5.77**	-1.61	
Owner	-0.05	0.42			1.12	**1.98**		
Other Large MSA Central city								
Non-owner	1.56	4.03			1.04	**4.49**		
Owner	-1.24	**7.09**			-0.15	0.64		
Central cities of Small MSA	1.76	**4.61**			**2.09**	**5.44**		
Non Central City in Large MSA	0.84				0.59			
Non Central City in Small MSA	0.96	**3.99** *			0.64	**4.66** *		
Other Places 10,000+	0.42				0.20			
Other areas	**-1.64**				-0.99			
West North Central								
Large MSA Central city								
Non-owner	**5.20**	5.47			**2.47**	**5.44**		
Owner	-0.53				-0.33			
Central cities of Small MSA	**1.82**	**4.85**			**1.90**	7.23		
Non Central City inLarge MSA	**1.09**				0.71			
Non Central City in Small MSA	0.22	**3.99** *			**1.64**	**4.66** *		
Other Places 10,000+	0.83				0.75			
Other areas	0.78				0.31			
West								
Mountain								
Large MSA Central city								
Non-owner	**4.65**	1.48			**5.03**	**4.61**		
Owner	**1.24**				0.98			
Central cities of Small MSA	**2.88**				**1.52**			
Non Central City in MSA	0.60	**7.39** *			0.75	**7.80** *		
Other Places 10,000+	1.22				**1.45**			
Other areas	**3.00**				**3.22**			
Pacific								
Non-owner								
Los Angeles/Long Beach CC's	**6.44**	**7.38**	**10.14**	6.29	**4.75**	**6.83**	**7.87**	**6.50**
Other Large MSA Central city	3.73				**3.72**			
Central cities of Small MSA								
Owner								
Los Angeles/Long Beach CC's	-0.35	**8.36**	2.01	3.10	**1.39**	**7.86**	**1.95**	**4.80**
Other Large MSA Central city	1.39				**1.39**			
Central cities of Small MSA								
Central cities of Small MSA	0.56				0.95			
Non Central City in Large MSA/PMSA	1.05	**14.32**	**5.65**	0.82	0.17	**16.37**	**6.94**	0.79
Non Central City in Small MSA	**2.90**				**3.15**			
Other Places 10,000+	**1.38**	**7.39** *		-3.22	**1.89**	**7.80** *		0.18
Other areas	**3.15**				**1.92**			
Reservation Indian				**12.72**				**12.72**

NOTE Bold indicates cell is significantly different from zero at 90% level Boxes show which cells were combined to form post-strata Asians are included in All Other when not separately shown
* indicates that the cell is combined with another non-adjacent cell

[*Received January 1992 Revised November 1992*]

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ESCAP MEETING NO. 1 - 12/08/99

MINUTES

Minutes of the Executive Steering Committee on
Accuracy and Coverage Evaluation (A.C.E.) Policy (ESCAP) # 1

December 8, 1999

Prepared by: Maria Urrutia and Genny Burns

The first meeting of the Executive Steering Committee on Accuracy and Coverage Evaluation (A.C.E.) Policy (ESCAP) was held on December 8, 1999 at 10:00 a.m.

Persons in attendance:

Kenneth Prewitt
William Barron
Paula Schneider
Cynthia Clark
Nancy Gordon
John Thompson
Jay Waite
Howard Hogan
Ruth Ann Killion
John Long
Susan Miskura
Raj Singh
Maria Urrutia
Genny Burns

I. Purpose of the group

The purpose of the group was briefly discussed. The charter is attached.

1. Provide advice to the Director of the Census Bureau on issues related to A.C.E. and adjustment.
2. Provide the Director and staff with background and related information on A.C.E.
3. Document rationale and reasons for decisions
4. Provide policy guidance on adjustment issues
5. Issue recommendation in February 2001 on whether adjusted counts should be released for redistricting.

II. Proposed agendas for first four meetings

The first three meetings will provide participants with a basic understanding of coverage measurement. After that, perhaps starting in the third meeting, the 1990 process will be reviewed. Topics included in this review will be the issues associated with the adjustment of the 1990 Census and its effect on the adjustment of the postcensal estimates.

Subsequently, relevant issues will be presented to the group as well as points of progress. John Thompson will work with Jay, Howard, and Susan to identify issues and will bring these to the committee.

III. Presentation - A.C.E. Design and Dual System Estimator (DSE)

Howard Hogan gave an overview of and responded to questions on the A.C.E. design and DSE. Handouts describing post enumeration surveys and the DSE in detail with formulae and text were distributed and are on file with these minutes.

The DSE is used for correcting the coverage error in the census. Aspects of the estimator are: DSE model in theory, application to census in general, and application to A.C.E. The basic model is as follows:

Basic DSE Model

List A (Census)	List B (A.C.E./PES)		Total
	In	Out	
In	N_{11}	N_{12}	$N_{1.}$
Out	N_{21}	N_{22}	$N_{2.}$
Total	$N_{.1}$	$N_{.2}$	$N_{..}$

Thus,

$$\frac{N_{11}}{N_{.1}} = \frac{N_{1.}}{N_{..}}$$

and

$$N_{..} = \frac{N_{.1} N_{1.}}{N_{11}}$$

where

$N_{1.}$ is the number of unique people correctly and completely enumerated in the census, $N_{.1}$ in the A.C.E., and N_{11} in both the census and A.C.E. An estimate of $N_{1.}$ is obtained from the census. Components of the DSE may include sampling and/or nonsampling errors.

The A.C.E. actually consists of two samples. The first is a sample of the population, known as the P sample, which measures omissions in the Census. The second is a sample of Census enumerations, known as the E sample. The E sample measures erroneous enumerations in the Census.

In 1980, the P and E samples did not overlap. In 1990, an overlapping sample design, based on the same blocks for both samples, was implemented.

To estimate the net undercount, it is critical to measure (i) the rate of erroneous enumerations in the initial phase of the census, and (ii) the rate of P sample matches to census enumerations in the A.C.E. block clusters. Followup operations will be used to determine erroneous enumerations by identifying duplicates, geocoding errors, fictitious persons, and illegible names. These operations will also be used to determine if a nonmatched person was correctly enumerated. All E sample matched cases will be assumed to be correct. E sample nonmatches will be followed-up to determine whether they were correctly enumerated. P sample nonmatches will be followed-up selectively.

For A.C.E., a sample of block clusters is selected averaging about 30 housing units each. Some blocks will have fewer than 30 housing units while others may be larger and require subsampling within the block. In September, 1999, maps were given to interviewers which contained only the physical boundaries but no housing units. Interviewers were required to map spot the location of each housing unit on these maps and also to complete independent listing books (ILBs) with housing unit information in each cluster. These ILBs were keyed and resulted in the sampling frame for A.C.E. interviewing. In July, 2000, the A.C.E. interviewers will visit each housing unit selected in A.C.E. sample to find out who lived there on April 1, 2000. This will give us the people in A.C.E. blocks which will then be linked with the person records from the census unedited file (CUF) that are in the E sample. This will not result in a one to one match since there will be some unmatched records in the P sample and some from the E sample. These unmatched census records will be sent to Field Division (FLD) for verification. This verification could be the third visit to a household since it could have previously received a census nonresponse followup and an A.C.E. interviewing visit.

For some people in either sample, the information collected will be insufficient, resulting in unresolved cases. For these cases, the probability of a match or correct enumeration will be assigned through estimation based on the corresponding rates from similar people with resolved status. A similar procedure will be used to handle mover cases. The hot-deck methodology to estimate missing characteristics, such as race, sex, and age, will be used. For whole household nonresponse, a weighting approach will be implemented.

In 1990, the population was divided into 357 poststrata or estimation cells to classify persons into groups that were as much alike as possible with respect to coverage error. Each person can belong to only one poststrata since they are mutually exclusive partitioned. For cases where the poststrata gets too small for estimation or publication of results, collapsing is implemented. Poststratification variables are being determined for the 2000 Census.

Synthetic estimation will be used in conjunction with the poststratification. Coverage factors will be computed using the following formula:

$$CF_j = \frac{N_{Tj}}{N_j^c}$$

= estimate of total population/complete census count (including erroneous enumerations and imputations)

where

j = poststrata numbers 1...n (For 1990, n=357)

These results for the j^{th} poststratum are applied to the census figures in the j^{th} stratum to form a synthetic estimate down to the block level. After adjustment, the numbers will not be integers. A controlled rounding will be used to obtain integer numbers such that each rounded number is within $\pm$ 1 of unrounded numbers.

IV. Future Discussions

The following topics were identified for further discussion in future meetings.

1. How the guidelines were developed for sending A.C.E. cases to followup.

2. The term, erroneous enumerations, includes some cases with insufficient information for matching and are not necessarily in error. Thus, this term may need to be changed for clarification in meaning.

V. Discussion Points for Next Meeting

1. How to operationalize the A.C.E. and DSE
2. How to measure each component of the DSE
3. More details on A.C.E. and DSE
4. What variables are used to impute unresolved cases.

<u>ESCAP Committee</u>

Kenneth Prewitt
William Barron
Nancy Potok
Paula Schneider
Cynthia Clark
Nancy Gordon
John Thompson, Chair
Jay Waite
Bob Fay
Howard Hogan
Ruth Ann Killion
John Long
Susan Miskura

cc:

Teresa Angueira
Ed Gore
Ed Pike
Catherine Miller
Fay Nash
Miguel Perez
Maria Urrutia
Genny Burns
Carolee Bush
Donna Kostanich
Raj Singh
David Whitford

ESCAP MEETING NO. 2 - 12/20/99

AGENDA

Kathleen P Zveare
12/16/1999 02:16 PM

To: Kenneth Prewitt, William G Barron Jr, Nancy A Potok, Paula Jane Schneider, Cynthia Z F Clark, Nancy M Gordon, John H Thompson, Preston J Waite, Robert E Fay III, Howard R Hogan, Ruth Ann Killion, John F Long, Susan Miskura, Kathleen P Zveare
cc: Maria E Urrutia, Fay F Nash, Phyllis A Bonnette, Patricia E Curran, Ellen Lee, Betty Ann Saucier, Jeannette D Greene, Margaret A Applekamp, Jane F Green, Sue A Kent, Mary A Cochran, Linda A Hiner, Carnelle E Sligh, Lois M Kline, Angela Frazier, Linda K Bonney, Carolee Bush, Rosalyn R Harrington, Geneva A Burns
Subject: Re[2]: Meetings for ESCAP

This message is to confirm the rescheduled ESCAP meeting.

Date: December 20, 1999

Time: 4-5 p.m.

Room: 2412/3

Agenda: Sample Design and Dual System Estimation

Attendees:

K. Prewitt
B. Barron
N. Potok
P. Schneider
C. Clark
N. Gordon
J. Thompson
J. Waite
B. Fay
C. Bush
H. Hogan
R. Killion
J. Long

Please cancel the 12/22 meeting. Thanks.

________________________________ Reply Separator
____
Subject: Re: Meetings for ESCAP
Author: Kathleen P Zveare at DMD
Date: 12/15/1999 11:04 AM

We are going to reschedule the next ESCAP meeting scheduled for Thursday December 22. Please let me know your availability for Monday December 20 from 4-5. Thanks.

________________________________ Reply Separator
____
Subject: Meetings for ESCAP
Author: Kathleen P Zveare at DMD
Date: 12/2/1999 10:00 AM

You should have received a memo (attached) letting you know that we would be contacting you about the Executive Steering Committee for A.C.E. Policy meetings.

The meetings will take place the 2nd and 4th Wednesdays starting December 8 from 10-11:30 in Rm. 2412/3.

Attendees:

K. Prewitt
B. Barron
N. Potok
P. Schneider
C. Clark
N. Gordon
J. Thompson
J. Waite
B. Fay
C. Bush
H. Hogan
R. Killion
J. Long
S. Miskura

ESCAP MEETING NO. 2 - 12/20/99

HANDOUTS

December, 1999

Objective for Choosing A.C.E. Post Stratification

1. We wish to group together people with similar census capture probabilities (gross undercount) [or similar A.C.E. probabilities]. This reduces correlation bias in dual system estimation (DSE).

2. We wish to group together people with similar net undercount. This is needed for synthetic estimation (carrying down).

3. The post strata should differentiate geographic areas. This is needed for synthetic estimation.

4. The expected sample size of each post strata should be larger than 100 in order to control variance and minimize ratio bias both in estimating the overall coverage rate and also the rate for movers under PES-C.

5. The post strata should be operationally feasible for both DSE estimation and for synthetic estimation. That is, the variables must be both timely and efficient in calculations for all blocks.

6. To the extent possible each person should be classified in the same post strata in both the census, and A.C.E. We wish to minimize classification error.

7. The post strata should take account of changes in census methods since 1990.

8. The strata and underling rational should be explainable to stakeholders. Changes from 1990 should be explainable.

9. The strata should not give the appearance of favoritism.

Small groups that cannot be assigned to a category based on data or logic should be assigned to the largest of the logical groups.

DRAFT
December 17, 1999

Recommendation for Poststratification for the Census 2000 Accuracy and Coverage Evaluation Survey

Richard Griffin, Donna Kostanich, and Dawn Haines
U.S. Bureau of the Census

I. Introduction

The goal of poststratification is to group together people who have similar coverage by the census. A common assumption is that people who share similar housing, similar language, similar cultural attitudes and similar education would also share similar census coverage. Race and ethnic origin often serve as a marker for these similarities.

This paper discusses a recommended poststratification plan for the Accuracy and Coverage Evaluation Survey (A.C.E.). Research is nearly complete but a **final decision on this recommendation has not been made.**

II. Background

The 2000 A.C.E. is different from the 1990 Post Enumeration Survey (PES). The A.C.E. will have approximately twice the sample size of the PES. A larger sample size will allow us to form more poststrata if we can determine a set of poststrata that will reduce correlation bias. In 2000, multiple responses to the race question will be permitted; in 1990, only one race could be selected.

The 1990 PES had 357 poststrata. The 357 design started with a cross-classification of the following variables: age/sex, race/Hispanic origin, tenure, urbanicity, and region. There were 840 cells in the cross-classification. Collapsing was necessary in order to produce poststrata with sufficient sample for reliable Dual System Estimation (DSE).

Race was the most important variable. After collapsing, five race/Hispanic origin poststrata were maintained: Non-Hispanic White and Other, Black, White or Other Hispanic, Asian and Pacific Islander, and Reservation Indians. Within each of these race/origin poststrata groups, seven age/sex categories were maintained. The other variables were collapsed as follows: first region, next urbanicity and if necessary tenure.

For American Indians residing on reservations all these variables were collapsed. Other American Indians were placed in the Non-Hispanic White and Other group unless they were Hispanic. Then they were placed in the Hispanic group. For Asian and Pacific Islanders region and urbanicity were collapsed and tenure maintained. For non-Black Hispanics and Blacks region was collapsed for two levels of urbanicity.

III. Recommended 2000 Poststrata Definitions

Schindler (2000) presents the results of the research documented in Griffin (1999). Major findings from this research are as follows:

- We have bias estimation concerns. The methodology is sound but there is concern about the large variance in the estimates of bias.

- The range of estimated mean square errors over the poststratification alternatives studied is not large. This is not surprising since all alternatives are reasonable and all are simulated using the same data set.

- The differences between the alternatives are not large and we have not done significance testing. Since the estimates are highly correlated, the variance of the differences should be small.

- There is little evidence that the increased variance in the 1990 PES 357 design resulting from having two geographic variables (urbanicity and region) was offset by decreased correlation bias enough to warrant poststratifying by both variables.

- Given the limitations of estimating bias and the limited set of potential poststratifying variables we could choose from, there was no indication that substantially increasing the amount of poststratification over the 1990 PES 357 design would improve correlation bias without adding too much variance. Even with the increased sample size for A.C.E. compared with the 1990 PES, there was no evidence that the mean square error of estimates would be reduced by dramatically increasing the amount of poststratification. There may be residual correlation bias in our recommended poststratification plan; however, we can find no additional variables for which, if used, we have evidence that the mean square error of estimates will be reduced. We can only use variables that are collected in Census 2000 and A.C.E.

- Type of Enumeration Area (TEA) was tested as a two level variable ((1) Tape Address Register and (2) Prelist Pocket, Update Leave, and List/Enumerate) using the 1990 PES data. It was not determined to be a significant variable. However, the definitions and distribution of TEA for Census 2000 is quite different from the 1990 Census. Thus we have decided to redefine the urbanicity variable to be a cross of urbanicity and type of enumeration area.

For A.C.E. poststratification, the recommended plan is to use the following variables:

- Race/Hispanic Origin - 7 categories
- Age/Sex - 7 categories
- Tenure - 2 categories
- Urbanicity/TEA - 4 categories
- Mail Response Rate - 2 categories

The seven Race/Hispanic Origin groups are:

- American Indian or Alaska Native in Indian Country
- Non-Hispanic American Indian or Alaska Native not in Indian Country
- Hispanic
- Non-Hispanic Black
- Non-Hispanic Native Hawaiian or Pacific Islander
- Non-Hispanic Asian
- Non-Hispanic White and Other

See Section IV. for the variable definitions and Section V. for further details on the Race/Hispanic Origin groups. The method and priority of collapsing, if necessary, has not yet been determined. Collapsing does not necessarily mean that an entire variable will be eliminated. For example, we could maintain Mail Response Rate for the Large Urban in Mailout/Mailback (MO/MB) category and eliminate Mail Response Rate for other levels of the Urbanicity/TEA variable.

We plan on maintaining all of the Race/Hispanic Origin, Age/Sex, and Tenure categories except for the non-Hispanic American Indian or Alaska Native not in Indian Country group. We currently expect that the Urbanicity/TEA and Mail Response Rate variables will be collapsed differently by Race/Hispanic Origin group as shown below.

- Non-Hispanic White and Other: No collapsing

- Non-Hispanic Black: Partial collapsing of the Mail Response Rate and Urbanicity/TEA variables

- Non-Hispanic Native Hawaiian or Pacific Islander: Eliminate the Mail Response Rate and Urbanicity/TEA variables (Tenure and Age/Sex only)

- Non-Hispanic Asian: Eliminate the Mail Response Rate and Urbanicity/TEA variables (Tenure and Age/Sex only)

- Hispanic: Partial collapsing of the Mail Response Rate and Urbanicity/TEA variables

- American Indian or Alaska Native in Indian Country: Eliminate the Mail Response Rate and Urbanicity/TEA variables (Tenure and Age/Sex only)

- Non-Hispanic American Indian or Alaska Native not in Indian Country: Poststratify by Tenure only

Depending on the actual A.C.E. sample sizes, additional collapsing may be necessary.

IV. Variable Definitions

The definition of the poststratification variables are included here. Section V. gives the details for assigning persons to a Race/Hispanic Origin group since this is more complicated as it depends on several variables and whether there are multiple race responses.

Major Race Groups: These are the 6 major groups. The specific collapsed races are shown in parentheses.

- White
- Black
- American Indian or Alaska Native
- Asian (Asian Indian, Chinese, Filipino, Japanese, Korean, Vietnamese, Other Asian)
- Native Hawaiian & Pacific Islanders (Native Hawaiian, Guamanian or Chamorro, Samoan, Other Pacific Islander)
- Other

Hispanic Origin: There are 2 major groups. The specific collapsed categories are shown in parentheses.

- No, not Hispanic
- Yes (Mexican, Puerto Rican, Cuban, Other Hispanic)

Age/Sex:

- Under 18
- 18 - 29 Male
- 18 - 29 Female
- 30 - 49 Male
- 30 - 49 Female
- 50 + Male
- 50 + Female

Tenure:

- Owner
- Non-owner

Urbanicity/Type of Enumeration Area (TEA)

- Mailout/Mailback (MO/MB) in urbanized areas 250,000 +
- MO/MB other urban
- MO/MB non-urban areas
- All other TEAs

Note: Urban/rural definitions will not be available in time for production poststratification for Census 2000 A.C.E. Thus, a revised definition will be needed.

Mail Response Rate: Mail response rate is a tract-level variable defined as the proportion of households in the 1990 mail universe which completed their census form without the aid of an enumerator. Low mail response rate tracts are those in the bottom 25^{th} percentile based on mail response rate.

- Low
- Other

Indian Country: A block-level variable indicating whether a block is (wholly/partially) inside an American Indian reservation/trust land, Tribal Jurisdiction Statistical Areas (TJSA), Tribal Designated Statistical Areas (TDSA), or Alaska Native Village Statistical Area (ANVSA).

V. Race and Hispanic Origin Classifications

The Census 2000 questionnaire has 15 possible race responses. The 15 responses are collapsed into 6 major race groups as shown below. Races which are collapsed into the major groups are shown in parentheses. Persons identifying themselves with a single race essentially place themselves into one of these 6 categories.

- White
- Black, African American, or Negro
- American Indian or Alaska Native
- Asian (Asian Indian, Chinese, Filipino, Japanese, Korean, Vietnamese, Other Asian)
- Native Hawaiian or Pacific Islander (Native Hawaiian, Guamanian or Chamorro, Samoan, Other Pacific Islander)
- Other

For the first time in census history, persons will be able to respond to more than one race category. Allowing persons to self-identify with multiple races results in many more than 6 race groups. In fact, after collapsing race to the 6 major groups, there are $2^6 - 1 = 63$ possible race combinations. It is necessary to subtract the 1 in this equation since each individual is assumed to have a race.

The race variable defined above is often cross-classified with the Hispanic origin variable to define poststrata. The Hispanic origin variable consists of two major groups. Categories which are collapsed into the major groups are shown in parentheses.

- No, not Spanish/Hispanic/Latino
- Yes (Spanish/Hispanic/Latino, Mexican, Mexican American, Chicano, Puerto Rican, Cuban, Other Spanish/Hispanic/Latino)

Combining the race and Hispanic origin variables yields $63 \times 2 = 126$ possible Race/Hispanic Origin groups. It is important to note that any poststratification scheme of interest cannot support 126 Race/Hispanic Origin poststrata. As a solution, these 126 Race/Hispanic Origin groups are assigned to one of seven Race/Hispanic Origin poststrata. The seven Race/Hispanic Origin poststrata are defined as follows:

- American Indian or Alaska Native in Indian Country
- Non-Hispanic American Indian or Alaska Native not in Indian Country
- Hispanic
- Non-Hispanic Black
- Non-Hispanic Native Hawaiian or Pacific Islander
- Non-Hispanic Asian
- Non-Hispanic White and Other

Note that missing race and Hispanic origin data are imputed. Rules for classifying the 126 race and Hispanic origin combinations into one of the seven Race/Hispanic Origin poststrata are now presented. Many of the decisions on how to classify multiple race persons are based on cultural, linguistic, and sociological factors which are known to affect coverage. The definitions are not necessarily data-driven and are subject to change.

A hierarchy is used to assign persons to a Race/Hispanic Origin group. The Race/Hispanic Origin designation occurs in the following order: American Indian or Alaska Native in Indian Country (IC), non-Hispanic American Indian or Alaska Native not in Indian Country (IC), Hispanic, Non-Hispanic Black, Non-Hispanic Native Hawaiian or Pacific Islander, Non-Hispanic Asian, and Non-Hispanic White and Other. Once a person is classified into a race/Hispanic origin group, they remain in that race/Hispanic origin group for poststratification purposes.

Tables 1 and 2 display the proposed assignment of Race/Hispanic Origin classification codes. Table 1 applies to Hispanic persons while Table 2 applies to non-Hispanic persons. The first seven rows of Tables 1 and 2 correspond to a single race response. The assignment of race/Hispanic origin is obvious in these cases. The remaining portion of the tables pertains to the assignment of multiple race responses to a single race/Hispanic origin category. Although a person may be associated with multiple race responses, each person is classified as belonging to only one race/Hispanic origin poststrata. All persons with a common number are assigned to the same race/Hispanic origin poststrata. Note that NH used in conjunction with Pacific Islander stands for non-Hawaiian Pacific Islander (not non-Hispanic). Following is a verbal description of who is assigned to each race/Hispanic origin group and their associated numeric code.

1: American Indian or Alaska Native in Indian Country: This group includes any person marking American Indian in IC either as their single race or as one of many races, regardless of their Hispanic origin.

2: Non-Hispanic American Indian or Alaska Native not in Indian Country: This group includes any non-Hispanic person marking American Indian or Alaska Native not in Indian Country as their single race.

3: Hispanic: This group includes all Hispanic persons who are not American Indians in IC. The last row of Table 1, All Other Groups and Two or More Races, includes all multi-race categories involving three or more races, excluding American Indians in IC. These persons are also classified as Hispanic.

4: Non-Hispanic Black: This group includes any non-Hispanic person who marks Black as their only race. It also includes the combination of Black and American Indian not in IC. In addition, people who mark Black and another single race group are coded as non-Hispanic Black.

5: Non-Hispanic Native Hawaiian or Pacific Islander: This category includes any non-Hispanic person marking only Hawaiian or non-Hawaiian Pacific Islander. It also includes the combination of Hawaiian and American Indians not in IC as well as non-Hawaiian Pacific Islander and American Indians not in IC. Also included is the combination of Hawaiian with non-Hawaiian Pacific Islander, Asian, White, or Other. Finally, non-Hawaiian Pacific Islanders combined with Asians results in a non-Hispanic Native Hawaiian or Pacific Islander classification.

6: Non-Hispanic Asian: This group includes any non-Hispanic person marking Asian as their single race. If Asian is combined with American Indians not in IC, they are also classified as non-Hispanic Asian.

7: Non-Hispanic White and Other: Non-Hispanic Whites and non-Hispanic Others are assigned to the non-Hispanic White and Other group. If American Indians not in IC are combined with White or Other, they are classified as non-Hispanic White and Other. If non-Hawaiian Pacific Islanders are combined with White or Other, they also fall into this group. Asian combined with White or Other are assigned to this group. Finally, all multi-race categories involving three or more races (except American Indians in IC) are classified as non-Hispanic White and Other.

Table 1: Possible Race/Hispanic Origin Classification Codes for Hispanic

		Not In IC	In IC
Single race:			
American Indian		3	1
Black		3	3
Hawaiian		3	3
NH-Pacific Islander		3	3
Asian		3	3
White		3	3
Other		3	3
American Indian and:	Black	3	1
	Hawaiian	3	1
	NH-Pacific Islander	3	1
	Asian	3	1
	White	3	1
	Other	3	1
Black and:	Hawaiian	3	3
	NH-Pacific Islander	3	3
	Asian	3	3
	White	3	3
	Other	3	3
Hawaiian and:	NH-Pacific Islander	3	3
	Asian	3	3
	White	3	3
	Other	3	3
NH-Pacific Islander and:	Asian	3	3
	White	3	3
	Other	3	3
Asian and:	White	3	3
	Other	3	3
American Indian and:	Two or More Races	3	1
All Other Groups and:	Two or More Races	3	3

Table 2: Possible Race/Hispanic Origin Classification Codes for Non-Hispanic

		Not in IC	In IC
Single race:			
American Indian		2	1
Black		4	4
Hawaiian		5	5
NH-Pacific Islander		5	5
Asian		6	6
White		7	7
Other		7	7
American Indian and:	Black	4	1
	Hawaiian	5	1
	NH-Pacific Islander	5	1
	Asian	6	1
	White	7	1
	Other	7	1
Black and:	Hawaiian	4	4
	NH-Pacific Islander	4	4
	Asian	4	4
	White	4	4
	Other	4	4
Hawaiian and:	NH-Pacific Islander	5	5
	Asian	5	5
	White	5	5
	Other	5	5
NH-Pacific Islander and:	Asian	5	5
	White	7	7
	Other	7	7
Asian and:	White	7	7
	Other	7	7
American Indian and:	Two or More Races	7	1
All Other Groups and:	Two or More Races	7	7

References

Griffin (1999), "Accuracy and Coverage Evaluation Survey: Poststratification Research Methodology", DSSD Census 2000 Procedures and Operations Memorandum Series #Q-5

Haines (1999), " Accuracy and Coverage Evaluation Survey: Poststratification Research Variables and Evaluation Statistics", DSSD Census 2000 Procedures and Operations Memorandum Series #Q-9

Schindler (2000), "Poststratification Options - Preliminary DSE Results"

Table 3: Pre-Collapsing Schematic for Poststratification Variables

	Low Mail Response Rate	Other Mail Response Rate
Non-Hispanic White and Other		
Owner		
Large Urban MO/MB	1	2
Other Urban MO/MB	3	4
Non-urban MO/MB	5	6
All other TEAs	7	8
Non-Owner		
Large Urban MO/MB	9	10
Other Urban MO/MB	11	12
Non-urban MO/MB	13	14
All other TEAs	15	16
Non-Hispanic Black		
Owner		
Large Urban MO/MB	17	18
Other Urban MO/MB	19	20
Non-urban MO/MB	21	22
All other TEAs	23	24
Non-Owner		
Large Urban MO/MB	25	26
Other Urban MO/MB	27	28
Non-urban MO/MB	29	30
All other TEAs	31	32
Non-Hispanic Native Hawaiian and Pacific Islander		
Owner	33	
Non-Owner	34	

Table 3: Pre-Collapsing Schematic for Poststratification Variables

	Low Mail Response Rate	Other Mail Response Rate
Non-Hispanic Asian		
Owner	35	
Non-Owner	36	
Hispanic		
Owner		
Large Urban MO/MB	37	38
Other Urban MO/MB	39	40
Non-urban MO/MB	41	42
All other TEAs	43	44
Non-Owner		
Large Urban MO/MB	45	46
Other Urban MO/MB	47	48
Non-urban MO/MB	49	50
All other TEAs	51	52
American Indian or Alaska Native in Indian Country		
Owner	53	
Non-Owner	54	

Note that connected cells may be collapsed together.

Age/Sex Groups:

	Male	Female
Under 18	A	
18 to 29	B	C
30 to 49	D	E
50 +	F	G

The following poststrata do not incorporate the seven age/sex groups:

Non-Hispanic American Indian or Alaska Native not in Indian Country		
Owner	55	
Non-Owner	56	

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POSTSTRATIFICATION OPTIONS - PRELIMINARY DSE RESULTS

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I. BACKGROUND

In order to determine an appropriate poststratification for the Census 2000 Accuracy and Coverage Evaluation (A.C.E.) survey, the Census Bureau has been evaluating data from the 1990 Census Post Enumeration Survey (PES). This paper discusses some of the results for fifteen poststratification options.

Two different logistic regression processes were employed using SAS and/or SUDAAN by Dawn Haines, Golam Farooque, and Roger Shores of Decennial Statistical Support Division (DSSD). The first was used to identify significant variables to determine the poststratification models. The significant variables included in these fifteen poststratification models were:

Race/Origin	R/O	(5 categories)
Age/Sex	A/S	(7 categories)
Tenure	Tenure	(2 categories)
Region	Region	(4 categories)
Regional Census Center	RCC	(13 categories)
Urbanicity	Urban	(3 categories)
Mail Return Rate	Mail	(2 categories - Block group level for PES data, but tract level for 1990 census counts)
Percentage Minority	Minority	(2 categories)

The second logistic regression was designed to develop the probability of capture for each person in the 1990 census. This when combined with an available probability of correct enumeration estimate allowed the calculation of population targets which are treated as if they were the truth. Three slightly different sets of target estimates were produced. These three models are referred to as SAS1, SAS2, and SUDAAN. The differences between SAS1 and SAS2 is in the rounding between two different versions of SAS. Except for the discussion of differences between the three models, all results are for SAS1.

Two versions of Demographic Analysis (DA) have been be incorporated into the target estimates. For the first version, the estimates for males were recalculated by multiplying the estimates for males by an adjustment factor such that the totals for six groups of males (Black 18-29, 30-49, 50+, non-Black 18-19, 30-49, 50+) will be equal to the corresponding total estimate for females times the DA sex ratios. Some potential problems were identified and the second version was developed with adjustment factors for both males and females. Thus, there are three sets of targets for each estimate, without DA, DA1, and DA2. At this time DA2 results are available for only the first seven poststratification models.

II. METHODOLOGY

The poststratification models are:

1. R/O, A/S, Tenure - 70 cells
2. R/O, A/S, Tenure, Region, Urbanicity (357 cells as in the 1990 PES)
3. R/O, A/S, Tenure, Region, Urbanicity, and Mail
4. R/O, A/S, Tenure, RCC, Urbanicity, and Mail
5. R/O, A/S, Tenure, Region, Mail, and Minority
6. R/O, A/S, Tenure, RCC, Mail, and Minority
7. R/O, A/S, Tenure, Urbanicity, Mail, and Minority
8. R/O, A/S, Tenure, Household Composition
9. R/O, A/S, Tenure, and Mail
10. R/O, A/S, Tenure, Region, and Mail
11. R/O, A/S, Tenure, and Urbanicity
12. R/O, A/S, Tenure, Region, and Minority
13. R/O, A/S, Tenure, Urbanicity, and Mail
14. CHAID2 - Race forced
15. CHAID3 - Tenure selected first

Options 3 through 7 required considerable collapsing, especially for minorities. The need for collapsing was usually determined by expected 2000 A.C.E. sample size but in a few cases by 1990 PES sample size. Collapse patterns were generally based on the relative significance of the variables for the particular race/origin group.

CHAID (Chi-Squared Automatic Interaction Detector) is a technique that sequentially segments a sample into heterogeneous groups, forming a tree-like structure of the data. In this application, the heterogeneity is determined using likelihood ratio tests of the equality of group capture probabilities. Independent variables, provided in the analysis, are used to form rules for dividing the sample. For ordinal independent variables, cutoffs are used to divide the data. For nominal independent variables, partitions of the unique values of the independent variable are used. The method is computer intensive, searching through each independent variable for one that produces maximal heterogeneity. The CHAID models were run by Don Malec.

Due to limitations in available software, independence between sample observations was assumed. Sample weights, normalized to the total sample size were used. A critical step in using CHAID is knowing when to stop segmenting the sample. For this application, a p-value of 0.001 or less was needed in to order to form a new segment. In addition, no segment was allowed to be form with a weighted (normalized) sample size smaller than 100.

Synthetic estimates were made for:

- 51 1990 poststratum groups

- 9 selected subtotal groups:
    - Non-Hispanic White/Other
    - Black
    - Hispanic White/Other
    - Asian and Pacific Islanders
    - American Indians on Reservations
    - Non-Black
    - Owner
    - Renter
- All 51 states
- 45 state sunnary, (excludes 6 rural states for which the target may be inappropriate because of high list/enumerate rates which are treated as low mail return rates.)
- 7 state sunnary grouped (census counts over 10 million).
- 23 state sunnary (census counts between 3 million and 10 million.)
- 23 state sunnary (including the District of Columbia) with census counts under 3 million.
- 16 selected cities (see Haines (1999))
- 436 congressional districts (including DC).

For each model estimates of standard error are made by a simple Jackknife procedure. These have also been adjusted for the larger Census 2000 A.C.E. sample sizes and the more equal weights.

For each of the three sets of targets, estimates of the BIAS^2 and MSE are made by:

$$BIAS^2(X) = (X - Target)^2 - Var(X - Target)$$

$$MSE(X) = (X - Target)^2 - Var(X - Target) + Var(X)$$

The estimates of bias squared and MSE are often negative. This occurs when the estimate of X is closer to the target than one standard error of (X-Target). A reasonable alternative, suggested by Lynn Stokes, is to set negative estimates of the bias squared to 0. This gives us:

$$BIAS'(X) = \sqrt{Max[(X - Target)^2 - Var(X - Target), 0]}$$

$$RMSE'(X) = \sqrt{Max[(X - Target)^2 - Var(X - Target), 0] + Var(X)}$$

Note that although individual negative estimates are improved by setting them equal to zero, averages of these values should be considered with caution as they may seriously overstate the average.

III. RESULTS

The results for the SAS1 target models are displayed in the attached Table 1 showing average values. The BIAS′ and RMSE′ estimates set negative estimates of $BIAS^2$ to 0. Estimates shown in **bold** are within 10% of the range (over all the models) of the lowest to highest value or are less than 110% of the lowest value.

Models 1, 8 and 9, the models with no geography, only race/origin, age/sex, and tenure and mail return rate or household composition, are the clear leaders for standard error for most categories. This is because the poststrata have no geographic limitations so the estimates for any area are based on all of the data available for the entire country. Conversely, the models using RCC use only a small portion of the data, especially for the Non-Hispanic White/Other group (no collapsing), so poststratification models 4 and 6 have the highest standard errors. Note that most of the average standard error for the 9 population subgroups is from the AIR population.

As far as MSE and RMSE' using the simple targets are concerned, there is no clear leader, but poststratification models 1, 5, 7, 8, and 9 may be slightly ahead of the others. Model 1, the minimal model, generally has the highest BIAS', but it is not very much higher than some of the other poststratification models. The BIAS estimates are favored for the models with the most variables since the logistic regressions used all of the variables except household composition. However, when the BIAS' is largest for states, it seems that the target value is at least as likely to be incorrect as the DSE. For example, the target coverage factors for some of the more rural states appear to be too large (AK 1.0440, ID 1.0284, ME 1.0188, MT 1.0369, VT 1.0393, WY 1.0264). This is apparently caused by the fact that these states had substantial list/enumerate populations in 1990, so most tracts were classified as low mail return. Therefore, the rural areas of these states received the increase in coverage factors associated with low mail return areas. These states have been omitted in one of the sub-tabulations of Table 1. Also, some of the target coverage factors for urbanized states appear to be too small (IL 1.0079, IN 1.0004, RI 0.9979). These states can contribute heavily to the average BIAS'. Since the logistic regression model for the targets was based on all of the variables used in the poststratification, it appears (with the usual caveats that this is 1990 data, that the targets are not the truth, etc etc etc) that the minimal model, despite its higher bias, does about as well as the more complex poststratification models for MSE at all of these levels. This can especially be seen from the sections of Table 1 for the 45 states where the six states for which the target estimates may be inappropriate have been removed.

Making the adjustments for the first attempt with Demographic Analysis tended to increase the estimates for Black males over age 18 by about 10%. The overall effect was to raise the national total by about 0.5%. The second attempt with DA had larger effects for black males under 50 and smaller effects for everyone else over 18. The overall effect was to raise the national total by about 1.0%. Thus, with the two DA targets, the poststratification options are compared to increasing levels of bias at the national level. More $BIAS^2$ estimates are now positive, especially for DA2. In general, as the bias gets larger, it dominates the variance and the seven poststratification options come closer together, so more of the estimates are close enough to the smallest estimate to be listed in **bold**.

Except for the national estimates for race/origin or tenure groups, the standard errors for Model 1 (sometimes called the minimal model) are the lowest of all with the rankings averaging very close to 0, and the average standard errors are substantially less than for the other poststratification models.

Target Model Variation

Three sets of P-Sample capture probabilities have been calculated, two using SAS and one using SUDAAN. The one discussed above used SAS and is referred to below as SAS-1. The targets using SAS-2 are almost always slightly less than those for SAS-1 (a total of 8,000 persons), but three-fourths of the difference is for Asians and Pacific Islanders. About two-thirds of the difference is for renters but there does not appear to be any geographic concentration. The SUDAAN based targets are about 26,000 persons less (about 0.01%%) than the SAS-1 targets, despite an increase of almost 7,000 AIR persons. Once again two-thirds of the difference is for renters. However, there is some geographic effect - Mississippi, North Carolina, and Oklahoma show increases. Illinois, Indiana, Michigan, Ohio, and Wisconsin show much larger than proportional decreases. Arizona and New Mexico despite having most of the AIR population show small decreases, so their non-AIR population probably show larger than proportional decreases. These results are shown in table 3.

In most cases there is little substantive change in the estimates of BIAS or MSE or BIAS' or MSE' for the three sets of target models. Even for the AIR population there is little difference despite the 1.6% difference between the SAS-1 and the SUDAAN targets because the $BIAS^2$ is so negative that BIAS' is 0 and MSE' is equal to the standard error.

Analysis

For the models examined, those which exclude geography and use only race/origin, age/sex, tenure and mail return rates or household composition, have several advantages. First, the standard errors are generally lower than for the other poststratification models. Standard errors will be released simultaneously with the results on April 1, 2001. Second, collapsing for Blacks and Hispanics would not be necessary. In 1990, the initial design calling for 1392 poststrata was not viable without the smoothing that was controversial. The revised design has 840 poststrata which were combined into 357. The minimal model would have only 84 poststrata; models 8 and 9 would have less than twice as many because of likely collapsing for the Asian, Pacific Islander, and American Indian poststrata. Poststratum sample sizes and collapse patterns for the models (excluding the regional census center models) are given in Table 2. The A.C.E. sample sizes will be about twice as large for whites, only minimally larger for Blacks (although there will be a shift from the large urban areas to the small urban and nonurban areas), a little less than twice as large for Hispanics, and substantially larger for Asians and American Indians.

The primary disadvantage of the models without geography is the potential for bias. Especially with the expected lower variances in the Census 2000 A.C.E, variance will make up a smaller part of the total mean square error. Thus, the models without geography are not quite as competitive for mean square error for the A.C.E. as they are for the PES. Another disadvantage is a fear factor. It appears that the use of geographic variables in 1990 added about as much variance as they reduced bias. This may not be so for Census 2000 especially if Master Address File (MAF) problems are concentrated in certain regions or types of areas.

As far as the other poststratification models are concerned, it seems that the less geography (either region or urbanicity) in the model, the better. The cross of region and urbanicity used in 1990 seems to have been excessive. At most one of these two variables should be used, probably urbanicity because state and congressional district estimates may have more than one urban type but they have only one region. Thus, urbanicity will often allow a larger proportion of the sample into synthetic estimates. In general, variables that allow most of the sample to go into the coverage factors for any synthetic estimate may be preferable to variables that result in the coverage factors for a synthetic estimate to be based on only part of the data. Examples of such variables would be the household composition (approximately, persons in nuclear family versus all other persons) variable which Population Division and others have advocated at times or the "associated with a mail return" variable which Population Division and Bill Bell of SRD have advocated. Unfortunately, despite their known values in predicting coverage, these variables may have substantial balancing problems that cannot be fully investigated and resolved quickly. Other variables, such as tract-level mail return rates, tract-level minority rates, tract-level ownership rates, or even tract-level nuclear family person rates might be useful for higher level estimates, but could greatly increase variances for estimates at the tract level and below where only a small portion of the total A.C.E. sample would be going into the estimates.

The two CHAID-based models show no improvement. In fact the model that does not force race shows considerable bias for the race groups. For example, the observed coverage factor for the AIR population is approximately 1.14 for the poststratification models but 1.06 for the CHAID model which does not force race.

All of the models change the results of the 357 poststratum design used by the 1990 PES. The following table shows the average of the absolute difference in the observed coverage factor from the 357 poststratum design for each of the models for the 51 PES poststratum groups, the 51 states, and the 436 congressional districts. Note that the models containing region may do doing better than the others because of the presence of one outlier block which pulled estimates down in the Northeast and because they are being compared with the 1990 PES 357 poststratum model which contained region.

		51 PES Groups	51 States	436 Cong. Districts
0	Target	0.009947	0.004809	0.005296
1	Minimal	0.014785	0.005088	0.005291
2	357 (RegUrb)	0.000000	0.000000	0.000000
3	RegUrbMail	0.004502	0.003059	0.002626
4	RccUrbMail	0.007146	0.006381	0.006360
5	RegMailMin	0.012225	0.003690	0.004015
6	RccMailMin	0.012549	0.007008	0.006451
7	UrbMailMin	0.012451	0.005756	0.005478
8	HHcomp	0.014730	0.005129	0.005341
9	Mail	0.014506	0.005042	0.005132
10	RegMail	0.011632	0.003672	0.004025
11	Urban	0.009424	0.004975	0.005174
12	RegMin	0.010999	0.003532	0.003792
13	UrbMail	0.010227	0.005593	0.005180
14	CHAIDrace	0.010521	0.005428	0.009651
15	CHAIDtenure	0.014336	0.005508	0.011028

Recommendation

The following recommendation is based on the patterns observed in the 1990 data and expectations for the Census 2000 A.C.E. data. It is not, and could not be, derived by a simple formula to choose the model with the lowest or highest value of some criterion. Non statistical considerations such as ease of explanation, acceptability to stakeholders, and prima facie fairness to all population groups had to be involved in the process. Taking these important considerations into account, there were two statistical considerations which seemed particularly relevant:

- There is some concern about geographic differences, especially as they may be affected by the quality of the MAF. It is believed that the urbanicity/Type of Enumeration variable may be able to capture some of any potential geographic differences.
- The tract level mail return rate seems to allow us to capture some of the socio-economic differences for synthetic estimates at lower levels of aggregation.

These two considerations lead to the 13th poststratification model in this study. The 1990 sample sizes, indicate that some collapsing may be necessary for Blacks and Hispanics, combining either the small urban and nonurban areas or combining the mail return variable within these areas, and that substantial collapsing will be required for the Asian, Native Hawaiian/Pacific Islander, and American Indian poststrata.

Table 1: Poststratification Model Averages

	PES 51 Minimal	357	RegUrb Mail	RccUrb Mail	RegMail Min	RccMail Min	UrbMail Min	HHComp	Mail	RegMail	Urban	RegMin	UrbMail	CHAID2	CHAID3
sc1990	**0.008052**	0.020298	0.018239	0.019653	0.011683	0.011663	0.012713	**0.007958**	**0.008298**	0.014147	0.013334	0.013956	0.013383	0.012794	0.011819
seace	**0.005601**	0.014816	0.013215	0.014609	0.008098	0.008089	0.009110	**0.005535**	**0.005788**	0.009812	0.009979	0.009723	0.009995	0.009390	0.008607
bias^2	0.000007	-0.000288	-0.000263	**-0.000440**	-0.000132	-0.000131	-0.000194	0.000021	0.000001	-0.000189	-0.000039	-0.000163	-0.000075	0.000079	0 000355
bias'	0.007333	**0.002784**	0.003738	0.004215	0.004931	0.005104	0.005385	0.007824	0.006946	0.004220	0.007206	0.004707	0.006050	0.008775	0.011112
mse90	0.000162	0.000357	0.000322	0.000349	**0.000095**	**0.000096**	**0.000119**	0.000163	0.000149	0.000148	0.000349	0.000148	0.000288	0.000398	0.000568
rmse90'	**0.013587**	0.021367	0.019904	0.021648	**0.014826**	0.014882	0.015878	**0.013727**	**0.013512**	0.016682	0.017523	0.016679	0.016769	0.018117	0.018669
mseace	0.000083	0.000146	0.000124	0.000128	**-0.000017**	**-0.000016**	**-0.000018**	0.000091	0.000074	**-0.000017**	0.000262	**-0.000000**	0.000201	0.000319	0.000503
rmseace'	**0.011552**	0.015906	0.014982	0.016812	**0.011674**	**0.011784**	0.012724	**0.011732**	**0.011427**	0.012700	0.014523	0.012954	0.013735	0.015291	0.016179
dabias^2	0.000219	-0.000157	-0.000148	**-0.000322**	0.000023	0.000021	-0.000024	0.000234	0.000226	-0.000033	0.000134	-0.000033	0.000094	0.000249	0.000515
dabias'	0.010554	**0.006732**	0.007481	0.007622	0.008768	0.009172	0.008788	0.010607	0.010488	0.008012	0.010500	0.007839	0.009726	0.011494	0.013990
damse90	0.000374	0.000488	0.000437	0.000468	**0.000250**	**0.000248**	0.000289	0.000376	0.000374	0.000304	0.000522	**0.000278**	0.000457	0.000568	0.000728
darmse90'	**0.016884**	0.023909	0.022386	0.023878	**0.017482**	**0.017586**	0.018968	**0.016852**	**0.016947**	0.019341	0.020500	0.018977	0.019998	0.020690	0.021050
damseace	0.000295	0.000277	0.000239	0.000246	**0.000138**	**0.000135**	**0.000152**	0.000304	0.000299	**0.000139**	0.000434	**0.000130**	0.000370	0.000489	0.000663
darmseace'	**0.014876**	0.018675	0.017726	0.019258	**0.014559**	**0.014741**	**0.015965**	**0.014872**	**0.014882**	**0.015597**	0.017711	**0.015292**	0.017155	0.017998	0.018918
da2bias^2	0.000427	0.000025	0.000006	**-0.000170**	0.000222	0.000214	0.000150								
da2bias'	0.014296	**0.010897**	**0.011350**	**0.011504**	0.013574	0.013496	**0.011850**								
da2mse90	0.000582	0.000670	0.000591	0.000619	**0.000448**	**0.000441**	**0.000464**								
da2rmse90'	**0.020107**	0.026628	0.025115	0.026572	**0.021306**	**0.021287**	**0.021749**								
da2mseace	0.000582	0.000670	0.000591	0.000619	**0.000448**	**0.000441**	**0.000464**								
da2rmseace'	**0.018243**	0.021799	0.020812	0.022338	**0.018614**	**0.018596**	**0.018844**								

	Minimal	9 Subtotals 357	RegUrb Mail	RccUrb Mail	RegMail Min	RccMail Min	UrbMail Min	HHcomp	Mail	RegMail	Urban	RegMin	UrbMail	CHAID2	CHAID3
se1990	0.011830	0.011298	0.013631	0.013701	0.011816	0.011817	0.013207	0.011242	0.011290	0.011386	0.011328	0.011294	0.011365	0.010367	0.006566
seace	0.007823	0.007467	0.008990	0.009036	0.007839	0.007839	0.008776	0.007415	0.007446	0.007518	0.007483	0.007451	0.007503	0.006858	0.004358
bias^2	-0.000454	-0.000425	-0.000641	-0.000639	-0.000451	-0.000450	-0.000638	-0.000425	-0.000427	-0.000426	-0.000423	-0.000423	-0.000425	-0.000332	0.000455
bias'	0.000000	0.000000	0.000000	0.000247	0.000000	0.000000	0.000000	0.000000	0.000000	0.000000	0.000000	0.000663	0.000000	0.000000	0.007613
mse90	0.000062	0.000017	0.000071	0.000073	0.000063	0.000064	0.000062	0.000017	0.000015	0.000018	0.000019	0.000019	0.000018	0.000027	0.000552
rmse90'	0.011830	0.011298	0.013631	0.013746	0.011816	0.011817	0.013207	0.011242	0.011290	0.011386	0.011328	0.011506	0.011365	0.010367	0.011534
mseace	-0.000212	-0.000219	-0.000309	-0.000307	-0.000210	-0.000209	-0.000308	-0.000219	-0.000221	-0.000219	-0.000217	-0.000217	-0.000218	-0.000164	0.000500
rmseace'	0.007823	0.007467	0.008990	0.009100	0.007839	0.007839	0.008776	0.007415	0.007446	0.007518	0.007483	0.007741	0.007503	0.006858	0.009954
dabias^2	-0.000378	-0.000350	-0.000594	-0.000597	-0.000376	-0.000378	-0.000581	-0.000343	-0.000337	-0.000347	-0.000353	-0.000362	-0.000350	-0.000247	0.000571
dabias'	0.004936	0.004565	0.004359	0.004094	0.005138	0.004735	0.004761	0.004844	0.005315	0.005006	0.004537	0.003920	0.004828	0.005500	0.012235
damse90	0.000138	0.000091	0.000118	0.000115	0.000139	0.000136	0.000119	0.000099	0.000106	0.000096	0.000089	0.000080	0.000093	0.000112	0.000669
darmse90'	0.015321	0.014445	0.016743	0.016635	0.015411	0.015147	0.016516	0.014688	0.015110	0.014888	0.014441	0.013943	0.014740	0.014277	0.014890
damseace	-0.000144	-0.000152	-0.000271	-0.000274	-0.000142	-0.000144	-0.000259	-0.000137	-0.000131	-0.000141	-0.000147	-0.000156	-0.000143	-0.000080	0.000617
darmseace'	0.011602	0.010878	0.012347	0.012180	0.011755	0.011439	0.012370	0.011133	0.011579	0.011326	0.010865	0.010316	0.011164	0.011118	0.013619
da2bias^2	-0.000239	-0.000206	-0.000489	-0.000497	-0.000234	-0.000240	-0.000466	0.000000							
da2bias'	0.010228	0.009938	0.009872	0.009466	0.010376	0.010109	0.010087	0.000000							
da2mse90	0.000277	0.000236	0.000223	0.000215	0.000281	0.000274	0.000234	0.000000							
da2rmse90'	0.020271	0.019385	0.021614	0.021261	0.020359	0.020103	0.021464	0.000000							
da2mseace	0.000277	0.000236	0.000223	0.000215	0.000281	0.000274	0.000234	0.000000							
da2rmseace'	0.016721	0.016018	0.017445	0.017064	0.016848	0.016587	0.017498	0.000000							

	51 States															
	Minimal	357	RegUrb Mail	RccUrb Mail	RegMail Min	RccMail Min	UrbMail Min	HHcomp	Mail	RegMail	Urban	RegMin	UrbMail	CHAID1	CHAID2
se1990	**0.002382**	0.004666	0.005686	0.006919	0.005008	0.006481	0.003569	**0.002377**	0.002918	0.005159	0.002747	0.005182	0.003456	0.004554	0.004606
seace	**0.001607**	0.003148	0.003850	0.004655	0.003377	0.004382	0.002411	**0.001605**	0.001975	0.003477	0.001859	0.003515	0.002342	0.003080	0.003127
bias^2	0.000063	0.000023	0.000017	0.000045	0.000036	0.000077	**0.000005**	0.000064	0.000025	0.000038	0.000049	0.000048	**0.000012**	0.000053	0.000061
bias'	0.004849	0.002739	**0.002178**	0.004524	0.002868	0.005388	**0.002499**	0.004888	0.003386	0.002795	0.004420	0.003624	0.002686	0.004181	0.005066
mse90	0.000070	0.000047	0.000063	0.000100	0.000070	0.000125	**0.000023**	0.000071	0.000036	0.000073	0.000058	0.000085	**0.000028**	0.000081	0.000090
rmse90'	0.006256	0.006149	0.006865	0.009504	0.006891	0.009714	**0.005188**	0.006288	**0.005137**	0.007011	0.006152	0.007594	**0.005187**	0.007415	0.007984
mseace	0.000066	0.000034	0.000038	0.000070	0.000052	0.000099	**0.000013**	0.000067	0.000030	0.000054	0.000053	0.000066	**0.000019**	0.000066	0.000074
rmseace'	0.005751	0.004922	0.005273	0.007696	0.005536	0.008134	**0.004229**	0.005788	**0.004481**	0.005590	0.005546	0.006199	**0.004287**	0.006279	0.006926
dabias^2	0.000134	0.000075	**0.000057**	0.000095	0.000100	0.000145	**0.000052**	0.000134	0.000091	0.000102	0.000108	0.000088	**0.000062**	0.000115	0.000100
dabias'	0.008403	0.005824	**0.005116**	0.006305	0.006945	0.007723	**0.005432**	0.008395	0.007300	0.006861	0.007622	0.005726	0.006079	0.007702	0.006922
damse90	0.000141	0.000098	0.000103	0.000150	0.000133	0.000192	**0.000070**	0.000141	0.000102	0.000137	0.000117	0.000125	**0.000078**	0.000143	0.000129
darmse90'	0.009300	**0.008153**	0.008827	0.010837	0.009607	0.011531	**0.007421**	0.009330	0.007806	0.008912	**0.008102**	**0.007791**	**0.007765**	0.009358	0.008292
damseace	0.000137	0.000086	0.000078	0.000120	0.000115	0.000167	**0.000060**	0.000137	0.000096	0.000118	0.000112	0.000105	**0.000069**	0.000128	0.000114
darmseace'	0.008930	**0.007176**	0.007475	0.009162	0.008562	0.010098	**0.006647**	0.008958	0.008058	0.008600	0.008244	0.007862	**0.007088**	0.009281	0.008750
da2bias^2	0.000281	0.000208	**0.000175**	0.000225	0.000239	0.000289	**0.000173**								
da2bias'	0.014287	0.012670	**0.011509**	**0.011214**	0.013353	0.012828	**0.011351**								
da2mse90	0.000288	0.000231	0.000220	0.000280	0.000272	0.000337	**0.000191**								
da2rmse90'	0.014688	0.013777	0.013874	0.014480	0.015076	0.015590	**0.012424**								
da2mseace	0.000288	0.000231	0.000220	0.000280	0.000272	0.000337	**0.000191**								
da2rmseace'	0.014482	0.013228	0.012934	0.013148	0 014357	0.014472	**0.011938**								

	45 States														
	Minimal	357	RegUrbMail	RccUrbMail	RegMailMin	RccMailMin	UrbMailMin	HHcomp	Mail	RegMail	Urban	RegMin	UrbMail	CHAID2	CHAID3
se1990	**0.002101**	0.004110	0.004437	0.006959	0.003980	0.006252	**0.002471**	**0.002381**	0.002595	0.005063	0.002715	0.005216	0.003004	0.004522	0.004599
seace	**0.001420**	0.002776	0.002996	0.004685	0.002682	0.004224	**0.001668**	**0.001591**	**0.001737**	0.003405	0.001821	0.003512	0.002016	0.003040	0.003111
bias^2	0.000007	0.000000	-0.000005	-0.000006	**-0.000009**	-0.000003	0.000003	0.000016	0.000011	-0.000005	0.000016	-0.000004	0.000014	0.000008	0.000018
bias'	0.002160	0.001540	0.000629	0.002837	**0.000313**	0.002273	0.001903	0.002972	0.002416	0.001002	0.002895	0.001630	0.002724	0.002253	0.003295
mse90	**0.000011**	0.000018	0.000015	0.000052	**0.000008**	0.000044	**0.000010**	0.000023	0.000019	0.000029	0.000024	0.000034	0.000025	0.000036	0.000048
rmse90'	**0.003590**	0.004739	0.004635	0.008203	0.004100	0.007227	**0.003739**	0.004537	0.004224	0.005624	0.004814	0.006024	0.004810	0.005835	0.006522
mseace	0.000009	0.000008	0.000004	0.000020	**-0.000001**	0.000018	0.000006	0.000019	0.000014	0.000011	0.000020	0.000014	0.000019	0.000021	0.000032
rmseace'	**0.003094**	0.003559	0.003253	0.006220	**0.002841**	0.005434	**0.003100**	0.003980	0.003567	0.004082	0.004148	0.004484	0.004046	0.004587	0.005363
dabias^2	0.000038	**0.000013**	**0.000011**	**0.000013**	0.000023	0.000020	0.000041	0.000066	0.000066	0.000041	0.000057	**0.000011**	0.000059	0.000049	0.000034
dabias'	0.005194	**0.003292**	**0.003105**	0.003851	0.004021	0.004114	0.005026	0.006269	0.006104	0.004887	0.005832	**0.003312**	0.005885	0.005556	0.004713
damse90	0.000043	**0.000030**	**0.000032**	0.000071	0.000039	0.000067	0.000047	0.000073	0.000074	0.000075	0.000065	0.000050	0.000070	0.000077	0.000064
darmse90'	**0.006075**	**0.005530**	**0.006038**	0.008774	0.006228	0.008506	0.006257	0.007313	0.006559	0.007104	0.006346	0.005576	0.007311	0.007367	0.006203
damseace	0.000040	**0.000020**	**0.000020**	0.000039	0.000030	0.000042	0.000044	0.000069	0.000069	0.000057	0.000061	0.000029	0.000064	0.000062	0.000048
darmseace'	0.005725	**0.004574**	**0.004938**	0.006969	0.005332	0.006932	0.005772	0.006898	0.006904	0.006802	0.006521	0.005688	0.006746	0.007312	0.006753
da2bias^2	0.000196	0.000164	**0.000123**	0.000156	0.000163	0.000188	0.000168								
da2bias'	0.012247	0.011473	0.010185	**0.009667**	0.011525	0.010635	0.011014								
da2mse90	0.000202	0.000186	**0.000169**	0.000212	0.000195	0.000233	**0.000181**								
da2rmse90'	**0.012688**	**0.012636**	**0.012749**	**0.013118**	0.013396	0.013574	**0.011984**								
da2mseace	0.000202	0.000186	**0.000169**	0.000212	0.000195	0.000233	**0.000181**								
da2rmseace'	**0.012460**	**0.012056**	**0.011740**	**0.011679**	**0.012623**	**0.012380**	**0.011561**								

		7 States > 10,000,000													
	Minimal	357	RegUrb Mail	RccUrb Mail	RegMail Min	RccMail Min	UrbMail Min	HHcomp	Mail	RegMail	Urban	RegMin	UrbMail	CHAID2	CHAID3
se1990	**0.002231**	0.004415	0.004105	0.005958	0.003789	0.005485	**0.002438**	**0.002245**	**0.002286**	0.004377	**0.002385**	0.004331	**0.002444**	0.003545	0.003536
seace	**0.001457**	0.002883	0.002667	0.003883	0.002456	0.003591	**0.001589**	**0.001465**	**0.001493**	0.002848	**0.001555**	0.002828	**0.001594**	0.002305	0.002301
bias^2	0.000007	-0.000004	-0.000009	**-0.000025**	-0.000012	**-0.000029**	-0.000000	0.000008	-0.000002	-0.000015	0.000010	-0.000005	-0.000001	0.000001	0.000004
bias'	0.002591	0.001042	0.000383	**0.000000**	**0.000000**	0.000375	0.001917	0.002644	0.001605	**0.000000**	0.002785	0.000822	0.001854	0.001583	0.001916
mse90	0.000012	0.000016	0.000009	0.000011	0.000003	**0.000002**	0.000006	0.000013	**0.000003**	0.000005	0.000016	0.000014	0.000005	0.000014	0.000017
rmse90'	0.004135	0.004827	0.004219	0.005958	0.003789	0.005575	**0.003717**	0.004197	**0.003389**	0.004377	0.004440	0.004538	**0.003668**	0.004326	0.004580
mseace	0.000010	0.000005	-0.000002	-0.000010	-0.000006	**-0.000016**	0.000002	0.000010	0.000000	-0.000006	0.000012	0.000003	0.000002	0.000006	0.000009
rmseace'	0.003547	0.003410	0.002821	0.003883	**0.002456**	0.003731	0.003020	0.003607	0.002709	0.002848	0.003812	0.003101	0.002968	0.003265	0.003552
dabias^2	0.000015	0.000021	0.000023	**-0.000013**	0.000023	0.000007	0.000018	0.000013	0.000018	0.000020	0.000017	0.000014	0.000018	0.000018	0.000018
dabias'	0.003055	0.003676	0.004059	**0.001494**	0.004411	0.002609	0.003386	0.002817	0.003424	0.003819	0.003261	0.002487	0.003467	0.002666	0.002683
damse90	**0.000020**	0.000041	0.000041	0.000023	0.000038	0.000038	0.000024	**0.000018**	0.000023	0.000040	0.000023	0.000034	0.000024	0.000032	0 000031
darmse90'	0.004263	0.006192	0.006278	0.006598	0.005956	0.006548	0.004650	0.004110	**0.003729**	0.006110	**0.003656**	0.005005	0.004701	0.004368	0.004159
damseace	0.000017	0.000029	0.000031	**0.000002**	0.000029	0.000020	0.000020	0.000015	0.000020	0.000028	0.000019	0.000022	0.000021	0.000024	0.000023
darmseace'	**0.003787**	0.005136	0.005310	0.004687	0.005148	0.004917	0.004147	**0.003605**	0.004125	0.005201	0.004029	0.004424	0.004214	0.004306	0.004301
da2bias^2	0.000118	0.000140	0.000149	**0.000094**	0.000152	0.000136	0.000131								
da2bias'	0.010046	0.011304	0.011785	**0.008407**	0.012246	0.011356	0.010842								
da2mse90	**0.000124**	0.000160	0.000166	**0.000130**	0.000167	0.000167	0.000137								
da2rmse90'	**0.010324**	0.012181	0.012530	**0.010987**	0.012834	0.012704	**0.011129**								
da2mseace	**0.000124**	0.000160	0.000166	**0.000130**	0.000167	0.000167	0.000137								
da2rmseace'	**0.010171**	0.011696	0.012106	**0.009762**	0.012493	0.011943	0.010968								

		21States	3-10 million												
	Minimal	357	RegUrb Mail	RccUrb Mail	RegMail Min	RccMail Min	UrbMail Min	HHcomp	Mail	RegMail	Urban	RegMin	UrbMail	CHAID2	CHAID3
se1990	0.002101	0.004110	0.004437	0.006959	0.003980	0.006252	0.002471	0.002077	0.002127	0.004066	0.002369	0.004147	0.002396	0.003616	0.003640
seace	0.001420	0.002776	0.002996	0.004685	0.002682	0.004224	0.001668	0.001405	0.001440	0.002736	0.001608	0.002797	0.001624	0.002436	0.002457
bias^2	0.000007	0.000000	-0.000005	-0.000006	-0.000009	-0.000003	0.000003	0.000007	0.000003	-0.000008	0.000009	0.000001	0.000003	-0.000003	0.000008
bias'	0.002160	0.001540	0.000629	0.002837	0.000313	0.002273	0.001903	0.002149	0.001687	0.000306	0.002227	0.001455	0.001785	0.000834	0.002312
mse90	0.000011	0.000018	0.000015	0.000052	0.000008	0.000044	0.000010	0.000011	0.000007	0.000009	0.000015	0.000019	0.000009	0.000010	0.000022
rmse90'	0.003590	0.004739	0.004635	0.008203	0.004100	0.007227	0.003739	0.003557	0.003229	0.004170	0.003943	0.004714	0.003605	0.004026	0.004757
mseace	0.000009	0.000008	0.000004	0.000020	-0.000001	0.000018	0.000006	0.000009	0.000005	-0.000001	0.000012	0.000009	0.000006	0.000003	0.000014
rmseace'	0.003094	0.003559	0.003253	0.006220	0.002841	0.005434	0.003100	0.003069	0.002686	0.002877	0.003361	0.003506	0.002978	0.002947	0.003841
dabias^2	0.000038	0.000013	0.000011	0.000013	0.000023	0.000020	0.000041	0.000039	0.000047	0.000021	0.000034	0.000002	0.000045	0.000034	0.000024
dabias'	0.005194	0.003292	0.003105	0.003851	0.004021	0.004114	0.005026	0.005271	0.005545	0.003891	0.004988	0.002119	0.005401	0.004854	0.003731
damse90	0.000043	0.000030	0.000032	0.000071	0.000039	0.000067	0.000047	0.000044	0.000051	0.000038	0.000040	0.000020	0.000051	0.000048	0.000038
darmse90'	0.006075	0.005530	0.006038	0.008774	0.006228	0.008506	0.006257	0.006148	0.005784	0.005699	0.005305	0.004064	0.006486	0.006187	0.005135
damseace	0.000040	0.000020	0.000020	0.000039	0.000030	0.000042	0.000044	0.000041	0.000049	0.000029	0.000037	0.000010	0.000048	0.000040	0.000030
darmseace'	0.005725	0.004574	0.004938	0.006969	0.005332	0.006932	0.005772	0.005807	0.006143	0.005240	0.005592	0.003935	0.006073	0.005973	0.005135
da2bias^2	0.000157	0.000121	0.000122	0.000126	0.000144	0.000134	0.000163								
da2bias'	0.011245	0.010700	0.010475	0.009282	0.011562	0.009471	0.011351								
da2mse90	0.000161	0.000139	0.000142	0.000184	0.000161	0.000181	0.000170								
da2rmse90'	0.011631	0.011516	0.011673	0.012539	0.012352	0.012360	0.011806								
da2mseace	0.000161	0.000139	0.000142	0.000184	0.000161	0.000181	0.000170								
da2rmseace'	0.011459	0.011083	0.011102	0.011196	0.011931	0.011178	0.011582								

	23 States	0-3 million												
	Minimal	357	RegUrb Mail	RccUrb Mail	RegMail Min	RccMail Min	UrbMail Min	HHcomp	Mail	RegMail	Urban	RegMin	UrbMail	CHAID2	CHAID3
se1990	**0.002685**	0.005250	0.007307	0.007176	0.006317	0.006992	0.004916	**0.002692**	0.003831	0.006395	0.003202	0.006386	0.004731	0.005717	0.005813
seace	**0.001823**	0.003569	0.004989	0.004863	0.004292	0.004767	0.003339	**0.001829**	0.002611	0.004345	0.002180	0.004378	0.003225	0.003903	0.003991
bias^2	0.000132	0.000053	0.000046	0.000113	0.000092	0.000183	**0.000009**	0.000133	0.000054	0.000097	0.000098	0.000108	**0.000023**	0.000121	0.000126
bias'	0.007992	0.004350	0.004138	0.007440	0.006073	0.009759	**0.003221**	0.008072	0.005478	0.005919	0.006920	0.006457	**0.003761**	0.008027	0.008539
mse90	0.000141	0.000083	0.000122	0.000171	0.000146	0.000236	**0.000040**	0.000142	0.000072	0.000153	0.000110	0.000167	0.000051	0.000167	0.000174
rmse90'	0.009336	0.007839	0.009707	0.011771	0.010382	0.013245	**0.006959**	0.009417	**0.007410**	0.010407	0.008690	0.011153	**0.007094**	0.011450	0.011967
mseace	0.000136	0.000067	0.000082	0.000139	0.000117	0.000208	**0.000023**	0.000137	0.000062	0.000123	0.000104	0.000136	**0.000036**	0.000142	0.000149
rmseace'	0.008848	0.006628	0.007863	0.010205	0.008934	0.011939	**0.005629**	0.008934	0.006659	0.008902	0.008068	0.009600	**0.005884**	0.010239	0.010770
dabias^2	0.000258	0.000148	0.000109	0.000203	0.000193	0.000301	**0.000074**	0.000257	0.000154	0.000202	0.000203	0.000188	**0.000091**	0.000219	0.000195
dabias'	0.012961	0.008791	0.007274	0.010011	0.010386	0.012573	**0.006425**	0.012945	0.010083	0.010498	0.011354	0.010006	0.007493	0.011835	0.011127
damse90	0.000267	0.000178	0.000186	0.000261	0.000248	0.000354	**0.000105**	0.000266	0.000173	0.000258	0.000216	0.000248	**0.000119**	0.000265	0.000242
darmse90'	0.013777	0.011145	0.012148	0.014011	0.013803	0.015809	**0.009327**	0.013825	0.010892	0.012698	0.012009	0.012041	**0.009864**	0.013771	0.012432
damseace	0.000262	0.000162	0.000146	0.000229	0.000219	0.000326	**0.000088**	0.000261	0.000163	0.000228	0.000209	0.000217	**0.000104**	0.000241	0.000218
darmseace'	0.013423	0.010173	0.010450	0.012526	0.012550	0.014565	**0.008207**	0.013465	0.011004	0.012701	0.011949	0.012493	**0.008889**	0.013816	0.013406
da2bias^2	0.000445	0.000308	0.000231	0.000356	0.000352	0.000478	**0.000194**								
da2bias'	0.018356	0.014886	**0.012368**	0.013832	0.015326	0.016340	**0.011506**								
da2mse90	0.000454	0.000337	0.000308	0.000414	0.000406	0.000531	**0.000226**								
da2rmse90'	0.018808	0.016328	0.016294	0.017316	0.018245	0.019417	**0.013382**								
damseace	0.000454	0.000337	0.000308	0.000414	0.000406	0.000531	**0.000226**								
da2rmseace'	0.018554	0.015652	0.014860	0.015960	0.017140	0.018250	**0.012557**								

	16 Cities														
	Minimal	357	RegUrb Mail	RccUrb Mail	RegMail Min	RccMail Min	UrbMail Min	HHcomp	Mail	RegMall	Urban	RegMin	UrbMail	CHAID2	CHAID3
se1990	**0.003530**	0.008303	0.007651	0.010321	0.006442	0.008153	0.005026	**0.003619**	**0.003739**	0.007176	0.004441	0.007360		0.006202	0.006828
seace	**0.002465**	0.005910	0.005448	0.006930	0.004549	0.005549	0.003503	**0.002527**	**0.002616**	0.005080	0.003109	0.005202		0.004341	0.004905
bias^2	0.000110	0.000047	0.000070	0.000054	0.000087	0.000085	0.000050	0.000108	0.000083	0.000091	0.000105	**0.000009**		0.000035	0.000089
bias'	0.005816	**0.003824**	**0.003851**	0.004069	0.005405	0.006032	0.004558	0.005948	0.005109	0.005481	0.005787	**0.003642**		0.004320	0.007533
mse90	0.000124	0.000122	0.000130	0.000182	0.000132	0.000160	**0.000081**	0.000122	0.000099	0.000151	0.000127	**0.000083**		**0.000087**	0.000142
rmse90'	**0.008035**	0.010168	0.010015	0.013035	0.009926	0.011943	**0.007813**	**0.008209**	**0.007546**	0.010473	0.008495	0.009401		0.008953	0.011356
mseace	0.000117	0.000087	0.000103	0.000106	0.000110	0.000118	0.000065	0.000115	0.000091	0.000122	0.000116	**0.000046**		0.000060	0.000118
rmseace'	**0.007294**	0.008161	0.008107	0.010015	0.008479	0.009860	**0.006691**	0.007410	**0.006781**	0.008911	0.007592	0.007602		0.007480	0.009979
dabias^2	0.000326	0.000337	0.000349	0.000286	0.000331	0.000310	0.000243	0.000290	0.000311					0.000122	**0.000062**
dabias'	0.013018	0.014030	0.013533	0.012218	0.012086	0.012692	0.011946	0.012022	0.012950					0.009635	**0.006040**
damse90	0.000340	0.000413	0.000409	0.000414	0.000376	0.000386	0.000273	0.000305	0.000327					0.000174	**0.000114**
darmse90'	0.013968	0.017417	0.016847	0.018395	0.015286	0.017104	0.013711	0.013156	0.014187					0.012491	**0.010260**
damseace	0.000333	0.000377	0.000381	0.000338	0.000354	0.000344	0.000258	0.000297	0.000319					0.000147	**0.000090**
darmseace'	0.013578	0.016015	0.015476	0.015843	0.013951	0.015253	0.013056	0.012701	0.013727					0.011413	**0.009021**
da2bias^2	0.000627	0.000682	0.000688	0.000592	0.000635	0.000600	**0.000537**								
da2bias'	0.020135	0.022350	0.021711	**0.018844**	0.020099	**0.018210**	**0.019292**								
da2mse90	0.000641	0.000758	0.000748	0.000720	0.000680	0.000675	**0.000567**								
da2rmse90'	**0.020902**	0.024238	0.023552	0.023600	**0.021897**	**0.022157**	**0.020567**								
damseace	0.000641	0.000758	0.000748	0.000720	0.000680	0.000675	**0.000567**								
da2rmseace'	**0.020610**	0.023327	0.022659	**0.021436**	**0.021126**	**0.020518**	**0.020074**								

Table 2: Poststratum 1990 PES P-Sample Sizes

						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
Minimal	White	owner				43882	12220	11775	27689	28302	28238	33611
		renter				17975	9449	9931	9924	9515	4386	6513
	Black	owner				9817	2909	2990	4428	5332	4588	6381
		renter				13977	3150	4432	3571	5178	1808	2824
	Hispanic	owner				6765	1840	1697	2592	2743	1786	2081
		renter				8642	2897	2739	2518	2541	815	1025
	API	owner				1345	387	382	688	792	526	578
		renter				851	429	399	464	474	171	156
	AIR	owner				998	215	213	286	294	233	262
		renter				705	107	127	167	193	25	62
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
357	White	owner	Northeast	large urban		3604	1287	1233	2359	2470	2548	3181
				small urban		2346	819	754	1524	1586	1793	2263
				nonurban		3416	842	766	2066	2070	1766	1927
			South	large urban		4015	1229	1234	2938	2996	2841	3596
				small urban		4053	1245	1230	2617	2738	3438	4580
				nonurban		4855	1387	1374	2895	3038	2973	3212
			Midwest	large urban		3505	1091	1061	2334	2365	2168	2699
				small urban		4499	1221	1164	2565	2626	2428	3041
				nonurban		3842	833	782	2056	2073	2172	2233
			West	large urban		3039	906	874	2357	2294	2159	2563
				small urban		3864	845	861	2325	2385	2281	2650
				nonurban		2844	515	442	1653	1661	1671	1666
		renter	Northeast	large urban		1334	914	980	1011	989	573	907
				small urban		1503	810	888	764	843	376	692
				nonurban		806	375	399	415	384	211	344
			South	large urban		1486	992	949	1138	1000	383	558
				small urban		2599	1354	1461	1201	1242	548	932
				nonurban		1425	578	583	639	600	316	372
			Midwest	large urban		994	723	723	643	633	273	433
				small urban		1730	941	1013	800	777	373	604

				nonurban		1078	351	397	424	412	196	265
			West	large urban		1342	913	965	1053	937	482	631
				small urban		2500	1100	1181	1216	1143	381	514
				nonurban		1178	398	392	620	555	274	261
	Black	owner	Northeast	large urban		1674	549	549	741	971	786	1104
			South	large urban		2798	841	816	1350	1633	1415	1967
			Midwest	large urban		1944	589	622	882	1046	938	1280
			West	large urban		603	160	180	311	354	305	392
				small urban		1954	537	585	813	963	879	1285
				nonurban		844	233	238	331	365	265	353
		renter	Northeast	large urban		2651	684	959	802	1110	427	671
			South	large urban		3164	753	956	896	1215	481	681
			Midwest	large urban		3358	641	1048	711	1285	353	588
			West	large urban		769	184	244	229	296	138	229
				small urban		3568	793	1073	813	1130	341	568
				nonurban		467	95	152	120	142	68	87
	Hispanic	owner	Northeast	large urban		384	120	122	194	186	112	146
			South	large urban		1504	470	470	694	753	580	682
			Midwest	large urban		364	129	94	133	134	95	103
			West	large urban		1884	507	463	673	720	407	473
				small urban		1814	459	403	615	660	403	490
				nonurban		815	155	145	283	290	189	187
		renter	Northeast	large urban		1472	471	490	450	547	154	207
			South	large urban		1669	565	538	521	569	227	326
			Midwest	large urban		784	273	240	221	197	57	55
			West	large urban		2103	786	709	661	635	181	221
				small urban		2039	641	611	492	483	142	167
				nonurban		575	161	151	173	110	54	49
	API	owner				1345	387	382	688	792	526	578
		renter				851	429	399	464	474	171	156
	AIR					1703	322	340	453	487	258	324
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
RegUrbMail	White	owner	Northeast	large urban	high mail	3321	1186	1138	2187	2268	2353	2935
					low mail	468	185	164	291	328	350	448

				small urban		2161	735	685	1405	1460	1638	2061
				nonurban	high mail	2323	608	527	1403	1424	1222	1323
					low mail	1093	234	239	663	646	544	604
			South	large urban	high mail	3786	1115	1127	2771	2838	2684	3396
					low mail	229	114	107	167	158	157	200
				small urban	high mail	3761	1119	1124	2438	2569	3231	4300
					low mail	292	126	106	179	169	207	280
				nonurban	high mail	4101	1181	1163	2466	2590	2535	2771
					low mail	754	206	211	429	448	438	441
			Midwest	large urban	high mail	3446	1069	1035	2280	2322	2098	2611
					low mail	358	118	129	200	211	175	226
				small urban		4200	1125	1061	2419	2458	2323	2903
				nonurban	high mail	3117	680	635	1654	1685	1591	1628
					low mail	725	153	147	402	388	581	605
			West	large urban	high mail	2899	836	815	2257	2215	2070	2464
					low mail	140	70	59	100	79	89	99
				small urban	high mail	3002	613	647	1773	1874	1805	2103
					low mail	862	232	214	552	511	476	547
				nonurban	high mail	815	164	144	502	525	539	500
					low mail	2029	351	298	1151	1136	1132	1166
		renter	Northeast	large urban	high mail	995	681	777	732	750	442	731
					low mail	582	432	386	442	388	203	291
				small urban		1260	611	705	601	694	304	577
				nonurban	high mail	458	230	262	260	235	141	198
					low mail	348	145	137	155	149	70	146
			South	large urban	high mail	1200	754	743	898	778	280	465
					low mail	286	238	206	240	222	103	93
				small urban	high mail	2149	1132	1211	1022	1046	471	803
					low mail	450	222	250	179	196	77	129
				nonurban	high mail	1107	450	454	505	467	256	311
					low mail	318	128	129	134	133	60	61
			Midwest	large urban	high mail	810	619	607	543	545	216	366
					low mail	424	219	230	205	176	98	105
				small urban		1490	826	899	695	689	332	566

				nonurban	high mail	794	275	301	301	310	150	205
					low mail	284	76	96	123	102	46	60
			West	large urban	high mail	1140	733	802	908	817	367	534
					low mail	202	180	163	145	120	115	97
				small urban	high mail	1674	732	802	817	793	309	455
					low mail	1089	474	492	535	475	142	119
				nonurban		915	292	279	484	430	204	201
	Black	owner	Northeast	large urban		1674	549	549	741	971	786	1104
			South	large urban		2798	841	816	1350	1633	1415	1967
			Midwest	large urban		1944	589	622	882	1046	938	1280
			West	large urban		603	160	180	311	354	305	392
				small urban		1954	537	585	813	963	879	1285
				nonurban		844	233	238	331	365	265	353
			Northeast	large urban		2651	684	959	802	1110	427	671
			South	large urban		3164	753	956	896	1215	481	681
			Midwest	large urban		3358	641	1048	711	1285	353	588
			West	large urban		769	184	244	229	296	138	229
				small urban		3568	793	1073	813	1130	341	568
				nonurban		467	95	152	120	142	68	87
	Hispanic	owner		large urban		4136	1226	1149	1694	1793	1194	1404
				small urban		1814	459	403	615	660	403	490
				nonurban		815	155	145	283	290	189	187
		renter		large urban		6028	2095	1977	1853	1948	619	809
				small urban		2039	641	611	492	483	142	167
				nonurban		575	161	151	173	110	54	49
	API	owner	Northeast			249	85	75	124	127	58	60
			South			214	42	61	114	132	50	56
			Midwest			132	46	35	49	68	43	40
			West			750	214	211	401	465	375	422
		renter	Northeast			187	120	86	129	119	36	32
			South			95	38	55	60	66	5	2
			Midwest			120	51	33	44	46	16	8
			West			449	220	225	231	243	114	114
	AIR	owner		large+small urban		165	47	43	40	52	35	26

			nonurban			833	168	170	246	242	198	236
		renter				705	107	127	167	193	25	62
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
RegMailMin	White	owner	Northeast	high mail	low min	7771	2505	2337	4971	5120	5144	6237
				all other		1595	443	416	978	1006	963	1134
			South	high mail	low min	11513	3374	3358	7559	7898	8265	10174
				all other		1410	487	480	891	874	987	1214
			Midwest	high mail	low min	10719	2849	2712	6302	6421	5926	7021
				all other		1127	296	295	653	643	842	952
			West	high mail	low min	6626	1571	1573	4462	4538	4304	4924
				all other		3121	695	604	1873	1802	1807	1955
		renter	Northeast	high mail	low min	2669	1506	1716	1564	1645	859	1458
				all other		974	593	551	626	571	301	485
			South	high mail	low min	4354	2306	2376	2371	2231	977	1536
				all other		1156	618	617	607	611	270	326
			Midwest	high mail	low min	3053	1694	1781	1507	1503	680	1122
				all other		749	321	352	360	319	162	180
			West	high mail	low min	2707	1427	1560	1671	1564	644	955
				all other		2313	984	978	1218	1071	493	451
	Black	owner	Northeast	high mail		921	297	301	442	599	432	616
				low mail		766	255	251	304	382	361	493
			South	high mail		2875	853	823	1326	1597	1295	1821
				low mail		699	208	216	321	378	349	462
			Midwest	high mail		2993	848	934	1326	1580	1389	1953
				low mail		946	285	280	385	440	452	634
			West	high mail		377	99	113	213	240	198	259
				low mail		240	64	72	111	116	112	143
		renter	Northeast	high mail		1177	299	420	327	495	182	268
				low mail		1684	445	613	557	683	264	430
			South	high mail		3786	879	1169	985	1382	492	786
				low mail		2774	602	821	646	916	351	489
			Midwest	high mail		1737	340	556	398	722	214	352
				low mail		1862	347	556	377	635	153	258
			West	high mail		448	137	151	166	185	74	122

				low mail		509	101	146	115	160	78	119
	Hispanic	owner		high mail		3411	839	827	1354	1454	799	900
				low mail		3354	1001	870	1238	1289	987	1181
		renter		high mail		3555	1316	1226	1163	1025	307	370
				low mail		5087	1581	1513	1355	1516	508	655
	API	owner			low min	1119	330	323	590	693	431	465
					high min	226	57	59	98	99	95	113
		renter			low min	590	253	241	298	321	125	119
					high min	261	176	158	166	153	46	37
	AIR	owner				998	215	213	286	294	233	262
		renter				705	107	127	167	193	25	62
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
UrbMailMin	White	owner	large urban	high mail	low min	13217	4094	4022	9291	9435	8855	10875
				all other		946	419	380	697	690	861	1164
			small urban	high mail	low min	13077	3576	3494	7990	8329	8917	11271
				all other		1685	554	515	1041	1006	1023	1263
			non urban	high mail	low min	10335	2629	2464	6013	6213	5867	6210
				all other		4622	948	900	2657	2629	2715	2828
		renter	large urban	high mail	low min	3910	2697	2830	2932	2732	1216	1979
				all other		1246	845	787	913	827	495	550
			small urban	high mail	low min	6255	3178	3477	2981	3075	1331	2321
				all other		2077	1027	1066	1000	930	347	421
			non urban	high mail	low min	2618	1058	1126	1200	1136	613	771
				all other		1869	644	645	898	815	384	471
	Black	owner	large urban	high mail		5029	1536	1547	2437	2999	2468	3452
				low mail		1990	603	620	847	1005	976	1291
			small urban	high mail		1498	392	456	618	743	647	937
				low mail		456	145	129	195	220	232	348
			non urban	high mail		639	169	168	252	274	199	260
				low mail		205	64	70	79	91	66	93
		renter	large urban	high mail		4738	1073	1559	1297	1989	727	1102
				low mail		5204	1189	1648	1341	1917	672	1067
			small urban	high mail		2159	525	653	493	707	190	363
				low mail		1409	268	420	320	423	151	205

			non urban	high mail		251	57	84	86	88	45	63
				low mail		216	38	68	34	54	23	24
	Hispanic	owner	large urban		low min	1771	477	493	789	860	457	548
					high min	2365	749	656	905	933	737	856
			small urban		low min	1036	248	227	360	376	215	233
					high min	778	211	176	255	284	188	257
			non urban		low min	604	114	107	205	218	127	119
					high min	211	41	38	78	72	62	68
		renter	large urban		low min	1912	727	679	702	659	188	252
					high min	4116	1368	1298	1151	1289	431	557
			small+nonurban		low min	1643	589	547	461	366	119	118
					high min	971	213	215	204	227	77	98
	API	owner			low min	973	270	264	501	598	327	351
					high min	372	117	118	187	194	199	227
		renter			low min	500	303	293	297	311	90	85
					high min	351	126	106	167	163	81	71
	AIR	owner	large+small urban			165	47	43	40	52	35	26
			nonurban			833	168	170	246	242	198	236
		renter				705	107	127	167	193	25	62
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
Mail	White	owner	high mail			36932	10431	10101	23555	24228	24089	28995
			low mail			6950	1789	1674	4134	4074	4149	4616
		renter	high mail			13077	7043	7563	7282	7124	3268	5211
			low mail			4898	2406	2368	2642	2391	1118	1302
	Black	owner	high mail			7166	2097	2171	3307	4016	3314	4649
			low mail			2651	812	819	1121	1316	1274	1732
		renter	high mail			7148	1655	2296	1876	2784	962	1528
			low mail			6829	1495	2136	1695	2394	846	1296
	Hispanic	owner	high mail			5359	1468	1385	2111	2235	1473	1707
			low mail			1406	372	312	481	508	313	374
		renter	high mail			5706	1869	1799	1684	1758	576	743
			low mail			2936	1028	940	834	783	239	282
	API	owner				1345	387	382	688	792	526	578
		renter				851	429	399	464	474	171	156

	AIR					1703	322	340	453	487	258	324
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
hhcomp	White	owner	hhcomp=1			27627	0	0	17755	20118	23798	26480
			hhcomp=2			16255	12220	11775	9934	8184	4440	7131
		renter	hhcomp=1			5572	0	0	3601	3899	3451	4964
			hhcomp=2			12403	9449	9931	6323	5616	935	1549
	Black	owner	hhcomp=1			2933	0	0	1732	2057	2780	3158
			hhcomp=2			6884	2909	2990	2696	3275	1808	3223
		renter	hhcomp=1			1513	0	0	847	925	1096	1345
			hhcomp=2			12464	3150	4432	2724	4253	712	1479
	Hispanic	owner	hhcomp=1			3132	0	0	1320	1566	1166	1148
			hhcomp=2			3633	1840	1697	1272	1177	620	933
		renter	hhcomp=1			1923	0	0	821	899	456	463
			hhcomp=2			6719	2897	2739	1697	1642	359	562
	API	owner				1345	387	382	688	792	526	578
		renter				851	429	399	464	474	171	156
	AIR					1703	322	340	453	487	258	324
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
Reg/Mail	White	owner	Northeast	high mail		7805	2529	2350	4995	5152	5213	6319
				low mail		1561	419	403	954	974	894	1052
			South	high mail		11648	3415	3414	7675	7997	8450	10467
				low mail		1275	446	424	775	775	802	921
			Midwest	high mail		10763	2874	2731	6353	6465	6012	7142
				low mail		1083	271	276	602	599	756	831
			West	high mail		6716	1613	1606	4532	4614	4414	5067
				low mail		3031	653	571	1803	1726	1697	1812
		renter	Northeast	high mail		2713	1522	1744	1593	1679	887	1506
				low mail		930	577	523	597	537	273	437
			South	high mail		4456	2336	2408	2425	2291	1007	1579
				low mail		1054	588	585	553	551	240	283
			Midwest	high mail		3094	1720	1807	1539	1544	698	1137
				low mail		708	295	326	328	278	144	165
			West	high mail		2814	1465	1604	1725	1610	676	989
				low mail		2206	946	934	1164	1025	461	417

	Black	owner	Northeast	high mail		921	297	301	442	599	432	616
				low mail		766	255	251	304	382	361	493
			South	high mail		2875	853	823	1326	1597	1295	1821
				low mail		699	208	216	321	378	349	462
			Midwest	high mail		2993	848	934	1326	1580	1389	1953
				low mail		946	285	280	385	440	452	634
			West	high mail		377	99	113	213	240	198	259
				low mail		240	64	72	111	116	112	143
		renter	Northeast	high mail		1177	299	420	327	495	182	268
				low mail		1684	445	613	557	683	264	430
			South	high mail		3786	879	1169	985	1382	492	786
				low mail		2774	602	821	646	916	351	489
			Midwest	high mail		1737	340	556	398	722	214	352
				low mail		1862	347	556	377	635	153	258
			West	high mail		448	137	151	166	185	74	122
				low mail		509	101	146	115	160	78	119
	Hispanic	owner	Northeast	high mail		300	92	90	158	160	89	107
				low mail		159	43	45	73	66	41	56
			South	high mail		2239	635	626	941	1008	721	861
				low mail		272	80	75	87	112	73	86
			Midwest	high mail		441	137	105	153	146	105	115
				low mail		86	40	21	31	34	26	37
			West	high mail		2379	604	564	859	921	558	624
				low mail		889	209	171	290	296	173	195
		renter	Northeast	high mail		847	256	292	258	325	88	111
				low mail		772	268	251	233	257	72	104
			South	high mail		2101	640	652	583	632	255	348
				low mail		496	214	170	157	142	49	63
			Midwest	high mail		463	162	147	133	116	34	37
				low mail		481	164	146	125	108	29	27
			West	high mail		2295	811	708	710	685	199	247
				low mail		1187	382	373	319	276	89	88
	API	owner				1345	387	382	688	792	526	578
		renter				851	429	399	464	474	171	156

	AIR					1703	322	340	453	487	258	324
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
Urban	White	owner	large urban			14163	4513	4402	9988	10125	9716	12039
			small urban			14762	4130	4009	9031	9335	9940	12534
			nonurban			14957	3577	3364	8670	8842	8582	9038
		renter	large urban			5156	3542	3617	3845	3559	1711	2529
			small urban			8332	4205	4543	3981	4005	1678	2742
			nonurban			4487	1702	1771	2098	1951	997	1242
	Black	owner	large urban			7019	2139	2167	3284	4004	3444	4743
			small urban			1954	537	585	813	963	879	1285
			nonurban			844	233	238	331	365	265	353
		renter	large urban			9942	2262	3207	2638	3906	1399	2169
			small urban			3568	793	1073	813	1130	341	568
			nonurban			467	95	152	120	142	68	87
	Hispanic	owner	large urban			4136	1226	1149	1694	1793	1194	1404
			small urban			1814	459	403	615	660	403	490
			nonurban			815	155	145	283	290	189	187
		renter	large urban			6028	2095	1977	1853	1948	619	809
			small urban			2039	641	611	492	483	142	167
			nonurban			575	161	151	173	110	54	49
	API	owner				1345	387	382	688	792	526	578
		renter				851	429	399	464	474	171	156
	AIR					1703	322	340	453	487	258	324
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
Reg/Min	White	owner	Northeast	Low Min		9286	2908	2723	5896	6055	5994	7231
				High Min		80	40	30	53	71	113	140
			South	Low Min		12748	3797	3763	8292	8645	9033	11041
				High Min		175	64	75	158	127	219	347
			Midwest	Low Min		11784	3112	2978	6888	7006	6649	7803
				High Min		62	33	29	67	58	119	170
			West	Low Min		9573	2190	2121	6202	6210	5957	6673
				High Min		174	76	56	133	130	154	206
		renter	Northeast	Low Min		3544	2059	2220	2129	2142	1117	1864
				High Min		99	40	47	61	74	43	79

			South	Low Min		5357	2854	2930	2893	2757	1205	1796
				High Min		153	70	63	85	85	42	66
			Midwest	Low Min		3735	1964	2082	1815	1760	808	1271
				High Min		67	51	51	52	62	34	31
			West	Low Min		4786	2342	2438	2777	2527	1086	1346
				High Min		234	69	100	112	108	51	60
	Black	owner	Northeast	Low Min		316	84	97	152	189	124	123
				High Min		1371	468	455	594	792	669	986
			South	Low Min		1024	261	271	438	510	275	328
				High Min		2550	800	768	1209	1465	1369	1955
			Midwest	Low Min		833	207	221	394	436	280	312
				High Min		3106	926	993	1317	1584	1561	2275
			West	Low Min		193	42	47	99	101	71	78
				High Min		424	121	138	225	255	239	324
		renter	Northeast	Low Min		613	169	208	220	232	50	78
				High Min		2248	575	825	664	946	396	620
			South	Low Min		1609	494	616	480	592	148	196
				High Min		4951	987	1374	1151	1706	695	1079
			Midwest	Low Min		428	118	136	118	168	41	73
				High Min		3171	569	976	657	1189	326	537
			West	Low Min		335	91	110	119	118	35	61
				High Min		622	147	187	162	227	117	180
	Hispanic	owner	Northeast	Low Min		285	84	83	145	141	76	91
				High Min		174	51	52	86	85	54	72
			South	Low Min		920	234	264	408	444	237	270
				High Min		1591	481	437	620	676	557	677
			Midwest	Low Min		352	106	83	123	118	79	92
				High Min		175	71	43	61	62	52	60
			West	Low Min		1854	415	397	678	751	407	447
				High Min		1414	398	338	471	466	324	372
		renter	Northeast	Low Min		619	239	229	232	232	56	73
				High Min		1000	285	314	259	350	104	142
			South	Low Min		958	396	381	314	277	86	119
				High Min		1639	458	441	426	497	218	292

			Midwest	Low Min		389	144	131	112	87	23	30
				High Min		555	182	162	146	137	40	34
			West	Low Min		1589	537	485	505	429	142	148
				High Min		1893	656	596	524	532	146	187
	API	owner				1345	387	382	688	792	526	578
		renter				851	429	399	464	474	171	156
	AIR					1703	322	340	453	487	258	324
						0-17	18-29M	18-29F	30-49M	30-49F	50+M	50+F
UrbMail	White	owner	large urban	high mail		13452	4206	4115	9495	9643	9205	11406
				low mail		711	307	287	493	482	511	633
			small urban	high mail		13124	3592	3517	8035	8361	8997	11367
				low mail		1638	538	492	996	974	943	1167
			nonurban	high mail		10356	2633	2469	6025	6224	5887	6222
				low mail		4601	944	895	2645	2618	2695	2816
		renter	large urban	high mail		4145	2787	2929	3081	2890	1305	2096
				low mail		1011	755	688	764	669	406	433
			small urban	high mail		6310	3195	3504	2999	3097	1346	2341
				low mail		2022	1010	1039	982	908	332	401
			nonurban	high mail		2622	1061	1130	1202	1137	617	774
				low mail		1865	641	641	896	814	380	468
	Black	owner	large urban	high mail		5029	1536	1547	2437	2999	2468	3452
				low mail		1990	603	620	847	1005	976	1291
			small urban	high mail		1498	392	456	618	743	647	937
				low mail		456	145	129	195	220	232	348
			nonurban	high mail		639	169	168	252	274	199	260
				low mail		205	64	70	79	91	66	93
		renter	large urban	high mail		4738	1073	1559	1297	1989	727	1102
				low mail		5204	1189	1648	1341	1917	672	1067
			small urban	high mail		2159	525	653	493	707	190	363
				low mail		1409	268	420	320	423	151	205
			nonurban	high mail		251	57	84	86	88	45	63
				low mail		216	38	68	34	54	23	24
	Hispanic	owner	large urban	high mail		3313	993	948	1420	1501	1026	1211
				low mail		823	233	201	274	292	168	193

			small urban	high mail		1547	375	332	524	552	346	401
				low mail		267	84	71	91	108	57	89
			nonurban	high mail		499	100	105	167	182	101	95
				low mail		316	55	40	116	108	88	92
		renter	large urban	high mail		3952	1331	1274	1250	1363	439	592
				low mail		2076	764	703	603	585	180	217
			small urban	high mail		1398	455	435	344	331	104	122
				low mail		641	186	176	148	152	38	45
			nonurban	high mail		356	83	90	90	64	33	29
				low mail		219	78	61	83	46	21	20
	API	owner				1345	387	382	688	792	526	578
		renter				851	429	399	464	474	171	156
	AIR					1703	322	340	453	487	258	324

TABLE 3: Target Values for Different Models

51 Poststratum Groups									
	Census	Estimates			Percent Differences		Coverage factors		
		SAS 1	SAS 2	SUDAAN	sas1-sud	sas2-sud	SAS 1	SAS 2	SUDAAN
1	15531971	15197471	15197420	15197306	0.001085%	0.000748%	0.978464	0.978460	0.978453
2	15786906	15832056	15831987	15831898	0.000999%	0.000564%	1.002860	1.002856	1.002850
3	14368932	14360486	14360443	14359267	0.008484%	0.008190%	0.999412	0.999409	0.999327
4	13155060	13176986	13176937	13176877	0.000834%	0.000458%	1.001667	1.001663	1.001658
5	4819780	4810777	4810761	4809781	0.020712%	0.020378%	0.998132	0.998129	0.997925
6	10647158	10663999	10663954	10661256	0.025724%	0.025305%	1.001582	1.001578	1.001324
7	10161672	10156777	10156748	10154280	0.024594%	0.024311%	0.999518	0.999515	0.999273
8	5702153	5680974	5680951	5679529	0.025446%	0.025046%	0.996286	0.996282	0.996032
9	8603396	8551650	8551608	8551550	0.001168%	0.000682%	0.993985	0.993981	0.993974
10	18267678	18205155	18205044	18204998	0.000861%	0.000251%	0.996577	0.996571	0.996569
11	13485679	13341753	13341696	13340190	0.011722%	0.011294%	0.989328	0.989323	0.989212
12	4534013	4603710	4603678	4603666	0.000948%	0.000259%	1.015372	1.015365	1.015362
13	6434135	6484258	6484187	6484089	0.002599%	0.001503%	1.007790	1.007779	1.007764
14	6813300	6989444	6989365	6989247	0.002818%	0.001681%	1.025853	1.025841	1.025824
15	4930780	5039251	5039210	5038068	0.023489%	0.022670%	1.021999	1.021990	1.021759
16	7128031	7330496	7330423	7330401	0.001297%	0.000306%	1.028404	1.028394	1.028391
17	2421312	2547541	2547513	2546100	0.056601%	0.055486%	1.052133	1.052121	1.051537
18	5043456	5200053	5199996	5197101	0.056806%	0.055714%	1.031050	1.031038	1.030464
19	4399385	4495267	4495231	4492734	0.056382%	0.055580%	1.021794	1.021786	1.021219
20	3136943	3249728	3249695	3247994	0.053375%	0.052361%	1.035954	1.035943	1.035401
21	1613862	1710635	1710610	1710599	0.002113%	0.000642%	1.059963	1.059948	1.059941
22	3848718	4133429	4133371	4133345	0.002032%	0.000632%	1.073976	1.073961	1.073954
23	2776979	2862539	2862509	2861740	0.027940%	0.026882%	1.030811	1.030800	1.030523
24	1312445	1430125	1430105	1430116	0.000613%	-0.000804%	1.089665	1.089649	1.089658
25	1788332	1807341	1807341	1807289	0.002898%	0.002898%	1.010629	1.010629	1.010600
26	3475035	3589016	3589016	3588949	0.001851%	0.001851%	1.032800	1.032800	1.032781
27	2144820	2178461	2178461	2177880	0.026705%	0.026705%	1.015685	1.015685	1.015414
28	937775	974467	974467	974439	0.002917%	0.002917%	1.039127	1.039127	1.039096
29	2497856	2500819	2500819	2499707	0.044480%	0.044480%	1.001186	1.001186	1.000741
30	2577204	2637951	2637951	2637899	0.001957%	0.001957%	1.023571	1.023571	1.023551
31	3123235	3447103	3447103	3447007	0.002806%	0.002806%	1.103696	1.103696	1.103665
32	4123140	4319949	4319949	4319815	0.003088%	0.003088%	1.047733	1.047733	1.047700
33	2500562	2618900	2618900	2617717	0.045221%	0.045221%	1.047325	1.047325	1.046851
34	1443654	1619235	1619235	1619187	0.002963%	0.002963%	1.121622	1.121622	1.121589
35	3140890	3310981	3310981	3308482	0.075519%	0.075519%	1.054154	1.054154	1.053358
36	969724	1022374	1022374	1022339	0.003398%	0.003398%	1.054294	1.054294	1.054258
37	682107	679483	679480	679460	0.003361%	0.002836%	0.996154	0.996148	0.996120
38	2157617	2189232	2189226	2189201	0.001427%	0.001152%	1.014653	1.014650	1.014638

39	529235	521568	521565	521463	0.020174%	0.019709%	0.985513	0.985508	0.985314
40	2917541	3013281	3013269	3013217	0.002128%	0.001716%	1.032815	1.032811	1.032793
41	2010444	2031931	2031922	2031169	0.037483%	0.037039%	1.010687	1.010683	1.010309
42	1114284	1128422	1128414	1128374	0.004282%	0.003543%	1.012688	1.012681	1.012645
43	2172461	2328489	2328466	2328298	0.008195%	0.007202%	1.071821	1.071810	1.071733
44	2040609	2235126	2235111	2235011	0.005140%	0.004459%	1.095323	1.095315	1.095267
45	609778	640607	640601	640277	0.051520%	0.050552%	1.050557	1.050547	1.050016
46	4004395	4329490	4329457	4329287	0.004690%	0.003918%	1.081185	1.081176	1.081134
47	1924008	2052989	2052965	2051423	0.076326%	0.075168%	1.067038	1.067025	1.066224
48	648494	716690	716678	716622	0.009599%	0.007875%	1.105161	1.105142	1.105055
49	4114526	4054627	4052335	4054310	0.007837%	-0.048715%	0.985442	0.984885	0.985365
50	3006209	3236109	3231583	3235572	0.016571%	-0.123295%	1.076475	1.074970	1.076297
51	434450	493667	493667	500595	-1.383920%	-1.383920%	1.136304	1.136304	1.152250
9 Race/Origin or Tenure Subtotals									
		Estimates			Percent Differences		Coverage Factors		
	CENSUS	SAS 1	SAS 2	SUDAAN	sas1-sud	sas2-sud	SAS 1	SAS 2	SUDAAN
nhw	184923744	186054560	186053443	186032131	0.012057%	0.011456%	1.006115	1.006109	1.005994
blck	28722227	30026596	30026596	30020709	0.019611%	0.019611%	1.045413	1.045413	1.045208
hisp	20810973	21867309	21867153	21863802	0.016037%	0.015324%	1.050759	1.050751	1.050590
api	7120735	7290736	7283918	7289882	0.011714%	-0.081817%	1.023874	1.022917	1.023754
air	434450	493667	493667	500595	-1.383920%	-1.383920%	1.136304	1.136304	1.152250
nblk	213289902	215706272	215698180	215686411	0.009208%	0.005457%	1.011329	1.011291	1.011236
U.S.	242012129	245732868	245724776	245707120	0.010479%	0.007186%	1.015374	1.015341	1.015268
own	162303028	162232435	162229535	162223671	0.005403%	0.003615%	0.999565	0.999547	0.999511
rent	79709101	83500433	83495241	83483449	0.020345%	0.014126%	1.047565	1.047499	1.047352
51 States									
		Estimates			Percent Differences		Coverage Factors		
	Census	SAS 1	SAS 2	SUDAAN	sas1-sud	sas2-sud	SAS 1	SAS 2	SUDAAN
AL	3948185	4002599	4002550	4001996	0.015055%	0.013835%	1.013782	1.013770	1.013629
AK	529342	552648	552624	552450	0.035902%	0.031658%	1.044028	1.043984	1.043653
AZ	3584545	3676071	3676002	3675781	0.007914%	0.006035%	1.025534	1.025514	1.025452
AR	2292393	2326075	2326046	2325723	0.015136%	0.013888%	1.014693	1.014680	1.014540
CA	29008161	29849297	29846877	29846788	0.008404%	0.000297%	1.028997	1.028913	1.028910
CO	3214922	3278372	3278303	3278006	0.011160%	0.009045%	1.019736	1.019715	1.019622
CT	3185949	3210138	3210063	3209610	0.016446%	0.014103%	1.007592	1.007569	1.007427
DE	646097	655099	655085	655058	0.006369%	0.004184%	1.013933	1.013911	1.013869
DC	565183	589616	589597	589599	0.002869%	-0.000385%	1.043230	1.043196	1.043200
FL	12630465	12836628	12836376	12836150	0.003727%	0.001758%	1.016323	1.016303	1.016285
GA	6304583	6417617	6417484	6416807	0.012622%	0.010551%	1.017929	1.017908	1.017800
HI	1070597	1085670	1085159	1085500	0.015607%	-0.031478%	1.014079	1.013601	1.013921
ID	985259	1013226	1013211	1013043	0.018101%	0.016552%	1.028386	1.028370	1.028200
IL	11143646	11231739	11231503	11228924	0.025063%	0.022968%	1.007905	1.007884	1.007653

IN	5382167	5384133	5384086	5382888	0.023137%	0.022265%	1.000365	1.000357	1.000134
IA	2677235	2682958	2682927	2682351	0.022618%	0.021478%	1.002138	1.002126	1.001911
KS	2394809	2410174	2410139	2409582	0.024557%	0.023104%	1.006416	1.006401	1.006169
KY	3584120	3626194	3626149	3625782	0.011372%	0.010125%	1.011739	1.011726	1.011624
LA	4107395	4184338	4184256	4183894	0.010602%	0.008661%	1.018733	1.018713	1.018625
ME	1190759	1213092	1213075	1212879	0.017539%	0.016168%	1.018755	1.018741	1.018576
MD	4667612	4746059	4745876	4745760	0.006303%	0.002438%	1.016807	1.016767	1.016743
MA	5802118	5848564	5848332	5847877	0.011745%	0.007769%	1.008005	1.007965	1.007887
MI	9083605	9152105	9151997	9150124	0.021648%	0.020475%	1.007541	1.007529	1.007323
MN	4257478	4276657	4276577	4275859	0.018664%	0.016792%	1.004505	1.004486	1.004317
MS	2503499	2550444	2550415	2551849	-0.055041%	-0.056190%	1.018752	1.018740	1.019313
MO	4971676	4996427	4996378	4995413	0.020301%	0.019319%	1.004978	1.004969	1.004775
MT	775318	803913	803903	803789	0.015446%	0.014206%	1.036882	1.036869	1.036722
NE	1530832	1550286	1550269	1549903	0.024750%	0.023631%	1.012708	1.012697	1.012458
NV	1177633	1212625	1212585	1212458	0.013768%	0.010472%	1.029714	1.029680	1.029572
NH	1077101	1096742	1096724	1096524	0.019860%	0.018174%	1.018235	1.018218	1.018033
NJ	7558820	7595268	7594946	7594868	0.005260%	0.001017%	1.004822	1.004779	1.004769
NM	1486262	1543354	1543333	1543095	0.016780%	0.015380%	1.038413	1.038399	1.038239
NY	17445190	17779036	17778025	17778011	0.005766%	0.000078%	1.019137	1.019079	1.019078
NC	6404167	6506218	6506115	6506962	-0.011427%	-0.013009%	1.015935	1.015919	1.016051
ND	614566	625848	625841	625648	0.031999%	0.030932%	1.018357	1.018347	1.018032
OH	10585664	10633203	10633098	10631018	0.020552%	0.019565%	1.004491	1.004481	1.004284
OK	3051908	3094724	3094658	3096334	-0.052023%	-0.054151%	1.014029	1.014008	1.014557
OR	2776116	2835245	2835169	2834869	0.013264%	0.010581%	1.021299	1.021272	1.021164
PA	11533219	11577298	11577082	11576449	0.007333%	0.005471%	1.003822	1.003803	1.003748
RI	964869	958031	957999	957972	0.006150%	0.002769%	0.992913	0.992880	0.992852
SC	3370160	3429990	3429945	3429522	0.013665%	0.012350%	1.017753	1.017740	1.017614
SD	670163	680451	680444	680232	0.032106%	0.031146%	1.015351	1.015341	1.015025
TN	4748056	4823775	4823705	4823316	0.009508%	0.008070%	1.015947	1.015933	1.015851
TX	16593063	16981692	16981174	16979775	0.011290%	0.008241%	1.023421	1.023390	1.023306
UT	1693802	1726704	1726665	1726587	0.006761%	0.004474%	1.019425	1.019402	1.019356
VT	541116	562384	562374	562296	0.015729%	0.013975%	1.039304	1.039286	1.039141
VA	5978058	6067536	6067321	6067093	0.007305%	0.003765%	1.014968	1.014932	1.014894
WA	4746161	4837918	4837719	4837544	0.007733%	0.003619%	1.019333	1.019291	1.019254
WV	1756566	1772147	1772126	1771907	0.013563%	0.012385%	1.008870	1.008858	1.008733
WI	4758171	4787472	4787406	4786289	0.024714%	0.023351%	1.006158	1.006144	1.005909
WY	443348	455069	455063	454966	0.022712%	0.021458%	1.026438	1.026425	1.026205

ESCAP MEETING NO. 2 - 12/20/99

MINUTES

Minutes of the Executive Steering Committee on
Accuracy and Coverage Evaluation (A.C.E.) Policy (ESCAP) # 2

December 20, 1999

Prepared by: Genny Burns and Kathy Stoner

The second meeting of the Executive Steering Committee on Accuracy and Coverage Evaluation Policy was held on December 20, 1999 at 4:00 p.m.

Persons in attendance:

Kenneth Prewitt
William Barron
Nancy Potok
Paula Schneider
Cynthia Clark
Nancy Gordon
John Thompson
Jay Waite
Bob Fay
Howard Hogan
John Long
Ruth Ann Killion
Donna Kostanich
Raj Singh
Carolee Bush
Genny Burns
Kathy Stoner

I. Presentation - A.C.E. Poststratification

Howard Hogan recommended that the agenda topic for this meeting be 2000 Census poststratification since this is an issue of immediate concern. He briefly discussed the history of poststratification and then described the proposed plan for 2000 along with the objectives for choosing poststratification variables. Input and guidance on policy issues in developing poststratification is needed from the committee. Handouts were distributed describing the objectives, recommendation, and background information on poststratification. These handouts are on file with the minutes.

The 1990 Post Enumeration Survey (PES) had a 357 poststrata design which started with a cross-classification of the age/sex, race/Hispanic origin, tenure, urbanicity, and region variables. The 2000 A.C.E. will have a larger sample size than 1990 which should allow for more poststrata and should result in smaller variances.

Poststratification serves dual purposes of grouping people to form estimation cells that lead to reduced correlation bias in the dual system estimation (DSE) and of grouping people with similar net undercount or coverage probabilities for synthetic estimation purposes, down to the block level. Thus, the poststrata should be operationally feasible for both DSE estimation and for synthetic estimation. Poststrata should differentiate geographic areas and are required to have a minimum population size to control variance and reduce ratio bias. Also, there needs to be a minimum sample size. If these minimum requirements are not met, groups will be collapsed according to expected sample size within a poststratum. Groups that cannot be assigned to a category based on data or logic will be assigned to the largest of the logical groups since the larger groups will usually be disaggregated on other characteristics so the bias and variance should be smaller.

It is important for poststrata to be defined on variables that are reported consistently in the Census and the A.C.E. The poststrata variables should be well documented and thoroughly discussed in advance of receiving the data. The recommended poststrata variables, based on research from 1990, are race/Hispanic origin, age/sex, tenure, urbanicity/type of enumeration area (TEA), and mail response rate. Since the urbanicity/TEA variable is the only explicit geographic variable included, there is concern about creating the potential for bias due to geographic variation in undercount. However, the decision to exclude other geographic variables is based on research from 1990 results where region was included but appeared to add about as much variance as it reduced bias.

In 1990, only one race could be selected by the respondents but in 2000, for the first time in census history, multiple responses to the race question will be permitted. The Census 2000 questionnaire has 15 possible race responses. For estimation purposes, the 15 responses will be collapsed into 6 major race groups for which persons with a single race essentially place themselves. Allowing persons to self-identify with multiple races complicates the details for assigning persons to a race/Hispanic origin group. Thus, a hierarchy is proposed to assign persons to one of 7 race/Hispanic origin poststrata. Although data from 1990 and Dress Rehearsal (DR) have been researched, many of the decisions on how to classify persons into one of the 7 poststrata must be based on previously observed demographic factors and professional judgment.

The DR revealed inconsistencies in reporting more than one race in A.C.E. and Census which led to the need for broad racial poststratification categories. Also, the A.C.E. sample size will only support a limited number of race/Hispanic categories.

A decision memorandum will be prepared announcing the poststratification design in advance of the 2000 Census implementation. The recommended design was discussed with selected members of the National Academy of Sciences (NAS) Expert Review Panel on Census 2000. It will be formally presented to the entire Panel at a future meeting.

II. Issues Regarding Poststratification and Multiple Race Groupings

John Thompson volunteered to summarize the issues and concerns brought up at the meeting and distribute these for comments and further discussions. These issues are described as follow:

N The mail response rate variable is different from the other variables recommended for poststratification since it is based on a Census operation attribute rather than a respondent attribute. This will be the first time a poststratification variable has the quality of being operationally dependent rather than respondent dependent. Since the Census Bureau has some control over this variable, it will be important to document that no purposeful influences are introduced into the coverage estimates.

N It is important that consistency underlie the definition of racial poststrata. For example, the rationale for collapsing options for Asians should be consistent with other groups.

N A proposal was made to treat Hawaiians in a manner similar to that of the American Indians. People reporting Hawaiian and one or more other races and who are Hawaiians living in Hawaii should be classified as Hawaiian while those not living in Hawaii should be treated as Pacific Islanders.

N Perhaps Dress Rehearsal data should be run to test mode effects on reporting multiple race for A.C.E. and census data collection. Look at consistency of responses across race groups to determine how much they vary.

N Collapsing guidelines are an important component of the DSE methodology and it is essential that these are well documented.

N There was concern expressed regarding the lack of geographic poststratification variables. Subsequent discussion resulted in a proposal to consider some regional poststratification variables in the next round of discussions.

III. Next Meeting

The next meeting will be held on Wednesday, January 5, 2000. Agenda topics will be treatment of movers and other differences between 1990 and 2000.

ESCAP Committee	cc:
Kenneth Prewitt	Teresa Angueira
William Barron	Ed Gore
Nancy Potok	Ed Pike
Paula Schneider	Catherine Miller
Cynthia Clark	Fay Nash
Nancy Gordon	Miguel Perez
John Thompson, Chair	Maria Urrutia
Jay Waite	Genny Burns
Bob Fay	Carolee Bush
Howard Hogan	Donna Kostanich
Ruth Ann Killion	Raj Singh
John Long	David Whitford
Susan Miskura	

ESCAP MEETING NO. 3 - 01/05/00

AGENDA

Kathleen P Zveare
01/04/2000 03:01 PM

To: Margaret A Applekamp, William G Barron Jr, Phyllis A Bonnette, Geneva A Burns, Carolee Bush, Elizabeth Centrella, Cynthia Z F Clark, Mary A Cochran, Patricia E Curran, Robert E Fay III, Angela Frazier, Nancy M Gordon, Jeannette D Greene, Linda A Hiner, Howard R Hogan, Sue A Kent, Ruth Ann Killion, Lois M Kline, John F Long, Susan Miskura, Nancy A Potok, Kenneth Prewitt, Betty Ann Saucier, Paula J Schneider, Rajendra P Singh, Carnelle E Sligh, John H Thompson, Maria E Urrutia, Preston J Waite
cc:
Subject: Agenda for tomorrow's ESCAP meeting

The agenda for tomorrow's ESCAP meeting scheduled from 11-12:30 in Rm. G-316/3 is as follows:

1. Treatment of Movers

2. Other Differences Between 1990 and 2000

ESCAP MEETING NO. 3 - 01/05/00

HANDOUTS

January 5, 2000

A.C.E. - PES
2000 - 1990 CHANGES

(Talking Points)

SAMPLING

*   **SAMPLE SIZE**–approximately 300,000 housing units vs. 170,000 in the 1990 PES.

*   **WITHIN STRATA SAMPLE**--Designed to have at least the same or better c.v. than in 1990.

*   **SAMPLING PROBABILITY--**Units in sample will have much closer to equal sampling probabilities than in 90. Minority groups will be only slightly differential and we're increasing the sampling rate in potential problem clusters.

*   **2 STAGE SAMPLE FOR SMALL BLOCKS**--Our small block sample in 1990 was a "clunky" operation. For 2000 it has been greatly refined.

*   **COVERAGE (GROUP QUARTERS)-** We are not including group quarters (college dorms, institutions, military reservations, etc.) in the A.C.E. universe. We did not include some of them in the 1990 PES. We feel we cannot do an adequate job measuring their coverage. We will use a "rigorous" enumeration methodology in the initial enumeration.

LISTING/HU MATCHING

*   **INITIAL HOUSING UNIT MATCHING AND SUBSAMPLING METHOD**--In 2000 we will match our A.C.E. address listing to the January, 2000 version of the Decennial Master Address File. This will make subsequent subsampling of large blocks much easier and less time consuming.

PERSON INTERVIEW

*   **AUTOMATED A.C.E. INSTRUMENTS--**A large reason we are able to deliver the coverage measurement products earlier than we did in 1990 is that we are doing the A.C.E. interview using laptop computers: Keying is no longer needed and quality control checks are quicker.

*   **A.C.E./NRFU OVERLAP-**We are doing A.C.E. "telephone CAPI" interviews while nonresponse followup is underway. In these we call mail respondents to the census who have given us their phone number and conduct an A.C.E. interview with them.

* **TIMING and STAFFING**--In 2000 we are allowing 6 weeks for FLD to complete the A.C.E. Person Interview and 2 week for the Nonresponse Conversion operation. In 1990 we interviewed for 6 weeks and nonresponse followup was an unplanned operation which did not immediately follow the interviewing.

Since the A.C.E. follows immediately after NRFU, we will use our best initial count nonresponse followup interviewers in A.C.E. interviewing.

The A.C.E. Person Followup in 2000 will be done over a months time as it was in 1990.

* **PES-C**--Movers were a problem in the 1990 PES when we tried to find the census questionnaire for in-movers. In 2000 we are planning a PES-C approach which uses in-mover counts and demographics but outmover match rates.

MATCHING

* **COMPUTER MATCHING**--Improvements have been made in address standardization and parameter estimation. In the latter, we've incorporated theoretical advances in record linkage models presented in recent statistical literature.

* **SEARCH AREA**--We are planning to have the cluster be the search area except in exceptional areas where we will do an additional targeted surrounding block search. In the 1990 PES the search area included the surrounding blocks.

* **CENTRALIZATION OF MATCHING AND PROCESSING OPERATIONS**–We are doing clerical matching at one site. It is essentially a paperless operation–maps, housing unit, and person information is accessed by computer. In 1990 we needed to access huge numbers of paper maps, address listing books, microfilm of census forms, and actual census and PES forms. This required a much larger staff which we dispersed throughout our 7 processing offices.

PERSON FOLLOWUP

* **A.C.E. FOLLOWUP CASES**– We have cut the percentage of A.C.E. cases that need to be followed up in the field. In our tests we gained quantitative evidence that we can trust many of our initial interviews--especially those with census household members.

ESTIMATION

* **POST STRATIFICATION METHOD**--We will use the same variables as in the 1990 PES and add a mail return variable and account for update/leave areas.

ESCAP MEETING NO. 3 - 01/05/00

MINUTES

Minutes of the Executive Steering Committee on Accuracy and Coverage Evaluation (A.C.E.) Policy (ESCAP) Meeting # 3

January 5, 2000

Prepared by: Maria Urrutia and Genny Burns

The third meeting of the Executive Steering Committee on Accuracy and Coverage Evaluation Policy was held on January 5, 2000 at 11:00.

Persons in attendance:

Kenneth Prewitt
William Barron
Nancy Potok
Paula Schneider
Cynthia Clark
Nancy Gordon
John Thompson
Jay Waite
Bob Fay
Howard Hogan
Raj Singh
Gregg Robinson
Signe Wetrogen
Carolee Bush
Maria Urrutia
Genny Burns

I. Part 1 of the Presentation - Movers

Howard Hogan discussed the plans for handling movers in the census and A.C.E. The census will count people who resided in housing units on April 1. The three to four month time span between the April 1 Census date and the June A.C.E. interviewing allows for moving activity. The people who are in the same unit on Census Day and at the time of the A.C.E. interview are more straightforward to match but the ones who are somewhere else are more difficult to process. Particularly difficult are those cases that include deaths or births, persons who emigrate or immigrate, and individuals who moved to or from group quarters (GQs). (A.C.E. includes only housing units, not GQs.)

The three procedures described below for capturing movers were discussed.

Procedure A is based on defining the residents of the A.C.E. sample housing units as of Census

Day. Therefore, respondents are asked to identify all persons who were living or staying in the sample housing unit on Census Day. These persons are then matched against names on the census questionnaire for the sample address. From this information, estimates of the number and percent matched for non-movers and out-movers are made. The advantage is that matching is simpler since it is performed at the sample addresses. The disadvantage is that movers may be understated. Estimates for movers will not be based on the complete mover universe and will be biased to some degree.

Procedure B is based on where the residents of the A.C.E. sample housing units actually lived on Census Day. Thus, current residents of the A.C.E. sample housing units are asked where they lived on Census Day. Those residents determined to be movers are matched at their Census Day addresses. This often involves determining (geocoding) the location of Census Day addresses in the Census records. The advantages are (1) since the actual movers are being interviewed, a lower nonresponse rate and potentially more accurate data are obtained; and (2) a more complete mover universe is incorporated into the A.C.E. process. The disadvantages are more complex matching, Census Day address recall biases for inmovers, and geocoding problems.

Procedure C is a two-step process as follows: (1) Determining the number of movers from the current residents of A.C.E. housing units, and (2) Estimating the match rate for movers based on the Procedure A interview. Therefore, the match rate is estimated by determining or reconstructing Census Day residents of the A.C.E. household and matching them to Census records. The advantage is that it produces good estimates of the number of movers. The disadvantage is the match rate may not be representative of the entire mover universe.

The person match rate for Procedure C is calculated as:

$$\frac{M_{NV} + \left(\frac{M_{OV}}{N_{OV}}\right) N_{IV}}{N_{NV} + N_{IV}}$$

where M_{NV} = the weighted number of matched nonmovers in the census

M_{OV} = the weighted number of matched outmovers in the census

N_{IV} = the weighted number of inmovers in A.C.E.

N_{OV} = the weighted number of outmovers in A.C.E.

N_{NV} = the weighted number of nonmovers in A.C.E.

Although Procedure B was used in 1990, it will not be applied in 2000 partly based on research conducted in Dress Rehearsal (DR) and because Procedure C fits better with the timing of the operational flow. There is no ideal method for handling movers. This is a very difficult part of DSE methodology. However, Procedure C is judged to be the best blend of operational feasibility and accuracy. As with all our procedures, this will be included in the A.C.E. evaluations.

II. Part 2 of the Presentation - Differences Between the 1990 PES and 2000 A.C.E.

Howard distributed the attached handout describing the 1990-2000 changes. The effects of these changes on design were briefly discussed and it was noted that these changes have resulted in modest improvements to the design. It was also noted that these changes have been discussed with the Bureau's statistical advisors.

The major points raised during the discussion were as follow:

(1) The sample size of approximately 300,000 housing units is larger than that in 1990. Since small blocks will undergo a two-stage sampling process, their weights should be better controlled than in 1990 and there should be fewer outlier clusters. The goals of this two-stage sampling process are to attempt to reduce the contribution of small clusters to the variance of the DSE and to ensure that the workload can be efficiently managed.

(2) Housing units in A.C.E. initial sample blocks have been independently listed and will be linked to the Decennial Master Address File (DMAF). A disadvantage of listing earlier than in 1990 is there are many changes in addresses between the listing phase and Census Day. An advantage is that it alerts us to problems sooner so geocoding problems, e.g., early evidence of A.C.E. geocoding error, can be addressed and potentially corrected. The initial housing unit match will be conducted in February but information from this process will not be released in order that the independence to the A.C.E. not be compromised.

(3) Unlike 1990, where matching processes were conducted in several processing offices, the matching operations will be centralized in one processing office in 2000. This should result in better control and more consistency over the matching, especially in handling difficult cases.

(4) The rules regarding which persons go to A.C.E. followup (FU) have been modified.

Detailed discussions of these rules were deferred to a future meeting where a complete presentation of the rationale for sending people to FU will be conducted.

(5) In 1990, permanent staff conducted the Nonresponse Conversion (NRCO) but in 2000 this process will be conducted by temporary interviewers.

III. Other

Howard mentioned that there is a change from the discussion two weeks ago in the grouping of American Indians. There were three stratum groups: (1) American Indians on reservations, (2) American Indians on tribal/trust lands, or (3) all other. It was decided to combine American Indians on tribal/trust lands with the all other grouping, i.e., groups 2 and 3 will be combined. Thus, the two strata will now be American Indians on reservations and those off reservations.

IV. Next Meeting

The next meeting will be held on Wednesday, January 12, 2000. Agenda topics will be 1990 evaluations of the PES and the associated decision processes. This discussion will include major issues and concerns with PES methodology and the steps that have been taken to address them.

A copy of the Federal Register Notice which documents the decision for not adjusting the 1990 Census will be distributed. Also, interest was expressed in viewing the training video on matching at a future meeting.

ESCAP Committee

Kenneth Prewitt
William Barron
Nancy Potok
Paula Schneider
Cynthia Clark
Nancy Gordon
John Thompson, Chair
Jay Waite
Bob Fay
Howard Hogan
Ruth Ann Killion
John Long
Susan Miskura

cc:

Teresa Angueira
Ed Gore
Ed Pike
Catherine Miller
Fay Nash
Miguel Perez
Maria Urrutia
Genny Burns
Carolee Bush
Donna Kostanich
Raj Singh
David Whitford
Gregg Robinson
Signe Wetrogen
Magdalena Ramos